TRACTS

ON THE NATURE

OF

ANIMALS AND VEGETABLES.

BY

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TO

JAMES EARL OF HOPETOUN,

VISCOUNT AITHRY, BARON HOPE,

LORD LIEUTENANT OF LINLITHGOWSHIRE,

ETC. ETC. ETC.

THIS TRANSLATION IS INSCRIBED.

TRANSLATOR's PREFACE

THE celebrity of Signor Spallanzani occasions our regret that we are so little acquainted with his works. This, which is now presented in our national language, is replete with matter so new, so singular and amusing, that I anticipate the pleasure every Philosophic reader must receive.

The treatise upon Animalcula I have ventured to abbreviate. Perhaps it will be esteemed a sufficient reason, that only the detailed method of executing some experiments, some letters corroborating the author's discoveries, and controversy with Mr Needham concerning the animation of animalcula, are omitted. All the experiments and the other treatises, are given as in the original. The degrees of heat are reduced from Reaumur's to Fahrenheit's thermometer. If inconsistencies appear, and I cannot say there are none, probably they arise either from the author using both thermometers.

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ters, and neglecting to remark it in his work, or to typographical error.

Animal Reproductions are not so generally known in Britain as they ought to be. M. Bonnet's treatise upon the reproduction of the head of the snail, is more concise than any I have hitherto seen. Signor Spallanzani (besides his *Prodromo*) has published two memoirs, *Sopra la riproduzione della testa nelle lumache terrestri*, which would with more propriety have formed a part of this volume: but I am not so entirely master of my own time, as the superintendance of publishing two or three hundred pages additional would require. Both memoirs are highly worthy of perusal.

Upon the merits of this translation, it becomes me to be silent. An anatomist will see where the words *axilla*, *cubitus*, and *radius*, might have been successfully used. Perhaps, there is not a greater impediment to the diffusion of science, than the multiplicity of technical terms, and the variety of nomenclature: yet all are of infinite use, when acquired. In some passages, I confess, I have not been able to ascertain the authors' meaning

with

with precision. The language of experiment is dull and uniform; and, unless a total change of arrangement, even of expression, is made, it is often impossible to translate with any semblance to elegance. Frequently, such a change cannot properly be made.—When all the requisites of the skilful translator of a scientific work are considered, it will not appear surprising, if there are errors here. The favourable, I may say the flattering reception, which my first juvenile performance has experienced in the world, induces me to hope that there are likewise some persons who may derive amusement from this.

Birns-House,
March 1799.

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OBSERVATIONS AND EXPERIMENTS

UPON THE

ANIMALCULA OF INFUSIONS.

CHAP. I.

To discover, whether long ebullition would injure or destroy the production of animalcula in vegetable infusions, I infused in distilled water, kidney beans, vetches, buckwheat, barley, maize, the seeds of mallows and beets; also the yolk of an egg. I took care that each species of seeds was from the same plant. The water used, was that in which seeds had been boiled, observing, that the water which had boiled half an hour, was taken to infuse seeds which had boiled half an hour. The vessels containing the infusions, were loosely stopped with corks.

Upon the 15th of September, I made thirty-two infusions, and, upon the 23d, examined them. There were animalcula in all; but the number and species differed in each. In the

maize

maize infuſions, the animalcula were proportionally ſmaller, and more rare, according to the duration of the boiling.

In the infuſion of kidney beans, boiled two hours, were three kinds of animalcula; very large; middle ſized; and very ſmall. The figure of the firſt partly elliptical, partly umbellated, attached to long filaments; the ſecond almoſt cylindrical; the third nearly ſpherical; all incredibly numerous.

In the infuſion boiled an hour and a half, were animalcula of the largeſt and ſmalleſt claſs, but few in number: ſtill fewer in that boiled an hour; and very few in that boiled half an hour.

The infuſion of mallow ſeeds, boiled two hours, produced middle-ſized circular animalcula, and others very large, the head extremity hooked. In two infuſions, boiled an hour and a half, the number and ſpecies of animalcula were the ſame: when boiled two hours, there were more, and as many as when boiled half an hour.

An immenſe number was in infuſions of vetches, boiled two hours. When boiled an hour and a half, they were very ſmall and rare, all ſemicircular, or bell-ſhaped. Some may be ſeen when the infuſion has boiled an hour; but it gives the eye pain to diſcover thoſe in an infuſion boiled half an hour.

In

In the infusion of barley, boiled two hours, were very large and numerous elliptical and oblong animalcula. Boiled an hour and a half, there was a moderate quantity of very minute animalcula. A few were observed, when boiled half an hour.

There was no fixed rule with the other infusions. In the buckwheat, boiled an hour and a half, were more animalcula than in the rest of the same grain. This happened with the beets and the egg: when boiled an hour, there were more animalcula than in other infusions of the same kind, boiled various durations of time. But in two infusions, boiled half an hour, there were fewer than in the others.

Thus, it clearly results, that long boiling the seed infusions, does not prevent the production of animalcula. To explain why the infusions boiled for the shortest time have the fewest animalcula, I may observe: That animalcula should appear in infusions, it is necessary the substances infused, sensibly begin to dissolve; for, as this dissolution is effected, or at least for a certain time, the number of animalcula augments. Seeds of plants, boiled for a shorter time, are, for a shorter period, encompassed and penetrated by the dissolving power of the fire; consequently, when put to macerate, will not be so soon decomposed. This is doubt-

leſs the cauſe why animalcula will ſooner be ſeen in infuſions of boiled, than unboiled ſeeds. A ſhort ebullition is not ſufficient to decompoſe the ſeeds of vegetables; for, decompoſition operates by long and gradual maceration.

Some days after making theſe experiments, the number of animalcula increaſed in the ſhorteſt boiled infuſions; and, towards the middle of October, the whole thirty-two infuſions were equally crowded. The only difference was in the figure, ſize, and motion of the animalcula.

Soon after, I made experiments, in the ſame manner, with peaſe, lentils, beans, and hemp-ſeed. The animalcula in all but the infuſion of beans, were more readily produced in the ſhorter boiled infuſions. To know whether augmenting the intenſity of the heat would obſtruct the production of animalcula, I ſlightly warmed, in a coffee roaſter, the eleven ſpecies of ſeeds, and then formed eleven infuſions. Neither the production nor the number was affected. They encreaſed, as uſual, from a ſmall number; and, in the middle of October, twenty days after the infuſion was made, they ſwarmed ſo much, that the fluid ſeemed perfectly animated.

I attempted to extend this experiment. I roaſted the ſeeds, and ground them as we do coffee: the appearance was like ſoot. I then made

made as many infuſions as there were kinds of ſeed; alſo infuſing the yolk of an egg, which had been expoſed to 279 of heat. Animalcula were equally produced from this powder; only the time was a little longer before the number was ſo great: but the weather was colder: and animalcula multiply much more quickly in warm weather.

Further, I ſubjected vegetable ſeeds to the moſt intenſe heat, the heat of burning coals, and the flame from a blowpipe. I expoſed the ſeeds in an iron plate upon coals. When converted to cinders, I reduced them to powder, and made as many infuſions as there were kinds of ſeed. The cinders from the blowpipe were extremely dry and hard. I could ſcarcely believe my eyes, when I ſaw animalcula in theſe infuſions.

CHAP. II.

I HERMETICALLY ſealed veſſels with the eleven kinds of ſeeds mentioned before. To prevent the rarefaction of the internal air, I diminiſhed the thickneſs of the necks of the veſſels, till they terminated in tubes almoſt capillary, and, putting the ſmalleſt part to the blowpipe, ſealed it inſtantaneouſly, ſo that the internal air underwent no alteration. It was neceſſary to

know whether the ſeeds might ſuffer by this incluſion, which might be an obſtacle to the production of animalcula. Other experiments had ſhewn me, 1. veſſels hermetically ſealed have no animalcula, unleſs they are very capacious: 2. animalcula are not always produced: 3. when they are produced, the number is never ſo great as in open veſſels. Although I uſed pretty large veſſels, two ſubſtances, peaſe and beans, had not a ſingle animalcule. The other nine afforded a ſufficient number; and to theſe I limited my experiments. I took nine veſſels with ſeeds, hermetically ſealed. I immerſed them in boiling water for half a minute. I immerſed other nine for a whole minute, nine more for a minute and a half, and nine for two minutes. Thus, I had thirty-ſix infuſions. That I might know the proper time to examine them, I made ſimilar infuſions in open veſſels, and, when theſe ſwarmed with animalcula, I opened thoſe hermetically ſealed. Upon breaking the ſeal of the firſt, I found the elaſticity of the air encreaſed. Seeds contain much air: a great quantity ſhould eſcape in their diſſolution, by heat or maceration, which muſt, of neceſſity, render the portion of included air denſer and more elaſtic. However, this elaſticity may originate partly from the elaſtic fluid diſcovered in vegetables, the nature of which is apparently different from the atmoſpheric

mofpheric fluid. I examined the infufions, and was furprifed to find fome of them an abfolute defart; others reduced to fuch a folitude, that but a few animalcula, like points, were feen, and their exiftence could be difcovered only with the greateft difficulty. The action of heat for one minute, was as injurious to the production of the animalcula, as of two. The feeds producing the inconceivably fmall animalcula, were, beans, vetches, buckwheat, mallows, maize, and lentils. I could never difcover the leaft animation in the other three infufions. I thence concluded, that the heat of boiling water for half a minute, was fatal to all animalcula of the largeft kind; even to the middle-fized, and the fmalleft, of thofe which I fhall term animalcula of the higher clafs, to ufe the energetic expreffion of M. Bonnet; while the heat of two minutes did not affect thofe I fhall place in the lower clafs.

Having hermetically fealed fix veffels, containing fix kinds of feeds producing animalcula of the lower clafs, I immerfed them in boiling water for two minutes and a half, three, three and a half, and four minutes. The feals of twenty-four veffels being broken at a fuitable time, there were no animalcula of the higher clafs feen, but more or fewer of the lower. The air was almoft always condenfed, both in this and in the other experiments.

In

In veſſels immerſed ſeven minutes, I found animalcula of the lower claſs. They appeared in veſſels immerſed twelve minutes.

The minuteneſs of animalcula of the lower claſs, does not prevent our diſtinguiſhing the difference of their figure and proportions.

Boiling half an hour, was no obſtacle to the production of animalcula of the lower claſs; but boiling for three quarters, or even leſs, deprived all the ſix infuſions of animalcula.

We know, that the heat of boiling water is about 212°. Theſe infuſions were of this heat at leaſt, as appeared by the marks they exhibited of ebullition, the whole time the ſurrounding water boiled. Philoſophers know, that water, boiled in a cloſe veſſel, acquires a greater degree of heat, than when boiled in an open. To know how much leſs than half a minute the boiling might be abridged, and animalcula of the higher claſs yet exiſt, I made uſe of a ſecond-pendulum, and immerſed the veſſels in boiling water for a given number of ſeconds, beginning with 29. In a word, boiling for a ſingle ſecond prevented their exiſtence; and I could only employ a degree of heat leſs than that of boiling water; for example, 209, 207, 205, 203°, deſcending to a degree which would not injure their production.

At a medium of 11°, I deſcended from 200, to 189, 178, 167°. Thus, I had four claſſes

of

of experiments, correſponding to the numbers 200, 189, 178, 167. In each claſs, were nine ſpecies of ſeeds, which made thirty-ſix veſſels. After the time neceſſary for the production of animalcula, I broke the hermetical ſeals, but ſaw none of the higher claſs at 167°. I continued this retrograde motion by 11°, and came to 155 and 110°; ſo that I had five claſſes of infuſions, and forty-five veſſels to examine. Not a ſingle animalcule was ſeen of the higher claſs, in veſſels hermetically ſealed, and expoſed to the moderate heat of 113°. This was during the middle of April: the thermometer in the ſhade ſtood at 88°. I took eighteen veſſels; nine had been expoſed to 99° of heat, and nine to 88°. No animalcula of the higher claſs were produced in the former; but I found them in the latter. In each veſſel, the quantity and kind of animalcula, as in veſſels not ſubjected to heat. The degree of heat fatal to them, was 92°.

Animalcula of the lower claſs, exiſt in ſealed veſſels expoſed to the heat of 212°; while thoſe of the higher claſs hardly appear at 92°: But, when produced, the ſame intenſity of heat that is fatal to the one, alſo deprives the other of life; and animalcula of the higher, as well as of the lower claſs, periſh at 106°, or at moſt at 108°.

Two

Two important consequences thence arise. The first evinces the extreme efficacy of heat to deprive infusions in close vessels of a multitude of animated beings; for, in open vessels, are always seen a vast concourse of animalcula. The second consequence, concerns the constancy of animalcula of the lower class appearing in infusions boiled in close vessels; and the heat of 212°, protracted an hour, has been no obstacle to their existence.

I have twice found animalcula produced in metal vessels sealed with metal, and kept half an hour in boiling water.

We are therefore induced to believe, that those animalcula originate from germs there included, which, for a certain time, withstand the effects of heat, but at length yield under it; and, since animalcula of the higher classes only exist when the heat is less intense, we must imagine they are much sooner affected by it, than those of the lower classes. Whence we should conclude, that this multitude of the superior animalcula, seen in the infusions of open vessels, exposed not only to the heat of boiling water, but to the flame of a blowpipe, appears there, not because their germs have withstood so great a degree of heat, but because new germs come to the infusions, after cessation of the heat.

CHAP.

CHAP. III.

In the month of May, I took the eggs of frogs from a ditch where they had been deposited by the mother a few hours before. I divided the quantity into equal portions, and exposed each to a different intensity of heat. The eggs were completely immersed in the water of a vessel, where I had put the ball of a thermometer. I then placed the vessel upon a slow fire, and, when the thermometer ascended to the requisite height, I took the eggs from the warm vessel, and put each portion in cold water. I had ten vessels, because I had ten portions exposed to different degrees of heat; 110, 122, 133, 144, 155, 167, 178, 189, 200, 212°. The eggs exposed to 110, 122, 133°, produced young. At 110°, almost all were fertile; there were fewer at 122°; and at 133°, the number was very small. At a greater heat, all the rest became putrid.

The heat neither accelerated nor retarded the production. Tadpoles were hatched in the same time as from those not exposed. The tadpoles all expired at 110°. The frogs which had afforded the eggs, all perished at 110°. They were middle-sized green-backed frogs, frequenting the ditches of plains and meadows.

I

I know that there are frogs which live in warm baths, although ſubject to heat greater than 110°. For inſtance, my friend Signior Cocchi relates, that frogs are not injured in the baths of Piſa at 115°; but they may perhaps be of a different ſpecies. Perhaps, after being long accuſtomed to a degree that would at firſt have been fatal, they no longer experience any bad effect. At leaſt we know, that men, who, the firſt time, can hardly endure the ſteam-bath ſix minutes, and perſpire violently whenever they enter; in a certain time ſupport it during fifteen minutes, without any ſenſible inconvenience.

The heat of 110° deſtroyed the nymphs and larvæ of muſkitoes; and 106° rat-tailed worms and water fleas. Water newts and leeches died at 110°; the eels of vinegar at 113. In my experiments upon the caterpillars of the elm-butterfly, and the larvæ of large blue fleſh-flies, the animals became reſtleſs at 93 and 95°: at 97°, they were much affected; and all died at 108. The eggs of theſe inſects long withſtood the impreſſion of heat: at 88°, they produced the greateſt poſſible number of worms: at 99°, they ſtill produced many. The number decreaſed with the heat, and none were produced at 144. The eggs of ſilk-worms had the ſame fate. The eggs of the blue fleſh-fly were very fertile at 124°; a ſmall number

number appeared at 135 and 137; none at 140°. The worms from the eggs became reſtleſs at 88°, and died at 108. Full grown worms of the ſame kind all died at 108. The flies of the ſame worms periſhed at 99. Some flies came from nymphs expoſed to 104 or 106°; none from thoſe to 110.

I expoſed grey peaſe, lentils, wheat, lintſeed, and the ſeed of trefoil, each to a different degree of heat, 167, 178, 189, 200, 212°. I ſowed each kind of theſe ſeeds ſeparately, in ſmall diſtinct ſpaces of earth, ſo prepared, that each ſpace might contain an equal number of ſeeds: 167° did not injure them; 178 begun to affect them, and very few were fertile. At 189°, there were produced only eleven plants of trefoil, and ten at 200°. Only three of thoſe at 212° germinated; ſome trefoil ſeeds could withſtand the heat of boiling water. The ſeeds were expoſed to heat in a ſand-bed: in a ſecond experiment, I put them in water, which I ſlowly heated, until it acquired the requiſite degree. By this means, the heat had greater influence upon the ſeeds. Peaſe, in this way, at 167°, germinated as abundantly as trefoil; but the lintſeed, lentils, and wheat, did not. At 189°, ſeven ſtalks of trefoil, and one of flax; at 200°, only ſix of trefoil; at 212°, none.

For thirteen days, I expoſed the plants to 167,

167, 178, 189, 200, 212° of heat, by dipping the branches in water gradually warmed. They were inſtantly replanted; but all died. I ſaw that 167° was a heat injurious to young plants: 155 and 144° did not hurt them, as, upon being replanted, all continued to vegetate.

I heated in ſand, as above, the following ſeeds: Beans, barley, white and black kidney beans, maize, vetches, parſley, ſpinage, beets, radiſhes, mallows. All expoſed to 167°, germinated; ſome at 178° begun to periſh; very few ſucceeded at 189 and 200°; and, at 212°, there appeared only one plant of white kidney beans, and three of beans. I repeated the experiment at 200, and 212°: not one germinated.

It is demonſtrated, by theſe experiments, that the eggs of the animals upon which they were made, withſtand heat better than the animals themſelves. Tadpoles and frogs periſhed at 110°; their eggs only at 133°; and ſome ſupported even a greater degree. Silk-worms and the elm-butterfly died at 108°: their eggs became ſterile only at 133. The large fleſh-flies periſhed at 99°; their nymphs at 110; their larvæ at 108; and their eggs at 140°. We remark almoſt the ſame relation between plants and their ſeeds, as between animals and their eggs. Some ſeeds, as trefoil, kidney beans, and beans, are fertile, though expoſed to 212°; while

while their plants cannot ſupport 167. The ſeeds of plants are more able to endure heat, than the eggs of animals. All the ſeeds upon which I made experiments by dry-warming, have been fertile, though expoſed to 167°, and ſome to 212; but no egg hatched after 144°.

Heat is much more noxious acting along with water.

The life of an animal concealed and concentrated in the integuments of an egg, is very faint, compared with that of an animal produced. During the firſt hours of incubation, the pulſation of the heart is the only indication of life in the chicken. Before the egg is hatched, its life is ſtill more faint; it is a *leſſer life*. Undoubtedly it is the ſame with that of the germs in the eggs of inſects, which have not the degree of heat neceſſary to hatch them. May this feebleneſs of the life of the embryo in the egg, be a reaſon why it is better able to ſupport heat in this ſtate, than when it is more expanded? It is certain, that minute animals, which have in this ſtate a life ſo feeble, which ſo little merits the name of life, reſiſt, with greater impunity, external accidents, as the intemperance of the air, than when they are more vivacious. If we cut off the head, take out the heart, or deprive a frog, toad, newt, ſnake, or viper, during winter, of ſome members, while they are torpid by cold, and ſeem more dead

dead than alive, they live much longer after such operations, than if they were to undergo them in the vigour of life: and I have observed, that insects immersed in water during winter, live longer than if immersed during summer.

The reason why seeds withstand heat better than eggs, rather seems to be, from the greater abundance of fluids in an egg, which, being expanded and put in motion by the heat, will violently operate against the subtile filaments of the germ, and occasion their rupture, and its destruction. Should the instance, of seeds resisting the influence of boiling water, induce us to think the germs of animalcula have the same faculty, the idea is corroborated by direct proofs, taken from the animals themselves. M. Duhamel observed a cockchafer live in a heat equal to that of boiling water; and, Schaeffer saw one species of caterpillar live at the same degree.

Carolina, and the Cape of Good Hope, swarm with animals of all figures and sizes, where the thermometer ascends in the shade to 110 and 122°. The direct heat of the sun is double, sometimes triple, in the warmest regions: that of the shade, at the Cape of Good Hope, is at least 189°, and in Carolina 212. If there are animals which sustain this heat, and their eggs preserve their fecundity; and, if

if there are animals in our climate, which can ſupport this degree of heat; why ſhall we deny, that there may be animalcula of the ſame conſtitution? M. Sonnerat ſaw fiſhes ſwimming in ſome waters in the Philippine Iſlands, where the thermometer ſtood at 187°.

The germs of the higher claſs of animalcula, however, cannot ſupport ſo great a heat as the animalcula themſelves. The animalcula die at 108°: the germs are not developed after 95°. We muſt thence conclude, that the nature of the germs of the higher claſs is very different from that of the lower, with regard to the power of reſiſting heat.

The difference in the eggs of other animals is more marked. The eggs depoſited by ſome butterflies upon the under ſide of leaves, like thoſe which certain inſects depoſit towards a northern aſpect, periſh at 79°; although this is 20° leſs than is neceſſary to hatch the eggs of other inſects; as of the aſili, which depoſit them in the ſkin of oxen or of cows; and thoſe flies, which inſinuate them into the frontal ſinus of ſheep, goats, and deer; or thoſe which depoſit them in the rectum of the horſe. We may ſay the ſame of the eggs of ſeveral which multiply in the human body, and in that of calves, where the heat is about 98°.

CHAP. IV.

I TRANSPORTED animalcula from the heat of the atmoſphere to the cold of an icehouſe. In the month of Auguſt, the thermometer in the open air ſtood at 83°, and in the icehouſe at 36. The only change I could obſerve, was, a ſlight relaxation in the motion of the animalcula; and they did not ſeem to ſuffer further, although expoſed to this degree of cold for ſeveral days. I then covered the veſſels with ice. At the beginning of the fourth day, a number of the animalcula died. Of twenty-two infuſions put in ice, thoſe of ſeven only remained alive. In eleven days, the animalcula in two of the ſeven had periſhed. After two months, thoſe in the other five were alive: one ſpecies appeared more numerous. Along with thoſe ſeven, I put in ice other two infuſions, yet ſterile, becauſe lately made; but, in a few days, they were filled with an army of the moſt minute animalcula.

During winter, I ſubjected the animalcula to new experiments. While the infuſions preſerved their fluidity, there was no particle of ice obſerved; which was occaſioned by the quantity of vegetable oil the infuſions contained, which ſecured them againſt freezing. Notwithſtanding

withſtanding the cold killed the animalcula of ſeveral infuſions, there were ſome more robuſt ſpecies that ſupported it. I took the advantage of a very cold day; and, although the thermometer fell to 13° under the freezing point, and the infuſions were covered with ice; upon breaking it, and preſenting ſome minute portions to the microſcope, I found living animalcula in the parts not completely congealed. They were immured in little grottoes of ice. In thoſe portions abſolutely frozen, the animalcula were dead. They did not revive upon melting the ice. The reſt retained their vivacity in the parts of the fluid not yet congealed [a]. I could not decide whether the animalcula periſh, becauſe the cold has deſtroyed them, or becauſe the infuſions have loſt their fluidity. It is a fact acknowledged by philoſophers, that water does not loſe its fluidity at 20, or even 22° below freezing, when at perfect reſt; which may be attained by incluſion in a cloſe veſſel, and removal from external motion. In this way, the animalcula ſurvived, although they ſuffered cold almoſt 20° below zero, in water not frozen. They ſwam about, but with a ſlower motion than uſual, while the thermometer

[a] Mr Muller of Copenhagen has diſcovered ſome ſpecies of animalcula which ſurvive congelation. I regret that his work did not come to my knowledge until the tranſcription of my manuſcript was finiſhed.

meter ſtood at 18° below the freezing point. This was the greateſt degree they could ſupport; as they died at 20°, although the water was not frozen, but was beginning to be covered by a thin cruſt. Two ſpecies ſurvived; and I may perhaps, or even without perhaps, ſay, they would have ſupported a more intenſe cold, had I been able to keep the water longer fluid.

I made infuſions ſimilar to the preceding, ſealed hermetically, and expoſed to cold, produced by a mixture of fine pounded ſea-ſalt, with ſnow. The thermometer deſcended to 34° below zero; and the infuſions were ſo much frozen, that they took half an hour to melt. But the germs of the animalcula were not injured, ſince the ſealed infuſions, at the ordinary time, afforded a number of animalcula. This fact well correſponds with what has been obſerved in thoſe inſects that have the greateſt analogy to animalcula. Some can ſupport 42° under freezing, others die at 22, or at moſt at 24°; a very great number cannot ſupport the cold of freezing, and others die at a degree far inferior. There is this difference between animalcula, and inſects expoſed to cold, that the former retain ſufficient life to uſe their members, whereas the latter immediately loſe their vivacity, and become motionleſs like dead bodies. Some inſects we may compare

to

to animalcula; for, besides the podura of Linnæus, which inhabits the snows of Sweden, I have observed the eels of vinegar preserve the motion of their members when exposed to a very intense degree of cold; for, although this fluid does not freeze so soon as water, the eels swim constantly while it is not frozen. Some kinds remain fluid 15° below freezing; other kinds remain fluid at 24°: but, when the cold is thus encreased, the eels become like animalcula insensibly motionless; but they move, although there is a thin crust of ice. As the congelation encreases, they continue motionless, extended in a straight line, or a little curved. If they receive sudden aid, by melting the vinegar, they will almost certainly be brought to life. If the ice is allowed to harden, it is impossible to revive them. These analogies between insects and animalcula, exist in the originating principle.

The winter of 1709 is celebrated for its cold, and the fatal effects it had upon plants and animals. The thermometer fell 33° below the freezing point. Who can believe, exclaims Boerhaave, that the eggs of insects were not entirely destroyed by this rigorous winter? After the spring begun to temperate the air, insects were at the ordinary time produced, the same as after the mildest winter. For five hours, I kept various eggs of insects, among

which were those of the elm butterfly, and silk-worms, enclosed in a glass vessel, in a mixture of ice and sal-gem, where the thermometer fell 38° below zero. All the eggs produced worms at the ordinary time.

The next year, by mixing ice and sal-gem with spirit of nitre, I obtained a degree of cold 56° under freezing. This did not injure the eggs of insects subjected to it. The result of all this is, that cold is less noxious to germs and eggs, than to animalcula and insects. Germs can, in general, support a degree of cold 33° under the freezing point. Some animalcula perish at freezing; others about 18° under it. The eggs of several insects sustained 56°; the insects they produced, only 16 or 18°: which I have remarked in silk-worms, and those of the elm butterfly. And, although there are caterpillars and chrysalids which withstand a very great degree of cold, their eggs will support a degree much more considerable. As animalcula and insects can less sustain heat than their eggs, so can they less sustain cold. What I said before, will apply here. The bodies of insects killed at 16 or 18° below freezing, are so indurated and frozen, that their members will not yield to the pressure of the finger, and seem perfect ice under the edge of the knife. This never happens to eggs exposed to a much more intense degree of cold.

 Their

Their humours preſerve their fluidity. They perhaps owe this advantage to the ſpirituous or oleaginous nature of their component parts, which may diminiſh the influence of the cold.

CHAP. V.

Man, like other animals, being ſubject to phyſical laws, is conſequently liable to periſh by an exceſs of heat or cold. However, he can ſuſtain both, when extended to a degree one would imagine inſupportable. It is commonly believed, with Boerhaave, that man cannot exiſt in air as hot as the blood. This rule had been eſtabliſhed by that philoſopher, from his obſerving, that certain birds and quadrupeds died in air heated to 149°, which is 53° greater than that of the human blood. But, ſuch an opinion appears ill-founded, ſince we know there are regions, inhabited, where the heat is greater than of the human blood. At the Cape of Good Hope, the thermometer ſtands in the ſhade at 113°. The heat of Carolina is greater than that of the human blood, ſince the thermometer falls, when taken from the ſhade, and put in a perſon's mouth. The heat of a warm bath ſometimes equals that of the hotteſt climates. There are waters which raiſe the

thermometer to 113, even to 122°[a]. Boerhaave thought the greatest degree of natural cold was zero in Fahrenheit's thermometer. He observed, that men, animals, and vegetables, perished at this degree. But we experience a much greater intensity of cold. For several winters, at Petersburgh, the thermometer fell to 60° below freezing, and once to 67°. The cold at Quebec was 72° below freezing: that at Torneao, observed by Maupertuis, 83°. But those high degrees of cold, although very intense when compared with that of our climates, are incommensurable with those that chill many places in Siberia. At Tomsk, Kirenga, Jeniseik, the thermometer falls 120°, 149, and even 157° under zero.

We cannot deny, that cold so piercing was pernicious, and indeed fatal, during 60° felt at Petersburgh: the face could not be kept uncovered above half a minute. At Torneao, where the thermometer fell to 83°, the breast felt as if lacerated. Some of the inhabitants of the cold climates lose their members in winter,

[a] Dr Fordyce supported 150° for twenty minutes without inconvenience, 198° for ten minutes, and 262° for eight. His respiration for seven minutes was not affected; the eighth, it became more frequent. He supported 220° a long time without inconvenience. A dog did not suffer from being exposed to 360°, in a basket, for thirty-two minutes.

ter, an arm or a leg. The cold experienced by Captain Middleton in Hudson's Bay, froze all the liquors except brandy; and the beds were covered with a coat of ice three inches thick, although the walls of the houses were of ſtone, and two feet thick; the windows very narrow, cloſed up with ſtrong boards; and although great fires were kept continually burning. The Dutch experienced a ſimilar cold in Nova Zembla. One, by keeping in motion, may ſupport a greater degree of cold. The ſavages of the moſt northern countries, hunt during the greateſt cold; and they know ſo well that motion alone can preſerve their lives, if any accident, during their expedition, ſhould threaten them with death, they accelerate it by reſt. From the narrative of ſome Dutchmen, who wintered at Spitzbergen in 78° of north latitude, we learn, that thoſe who ſhut themſelves up in wooden huts, one after another died with cold before the fire; whereas, thoſe who lived in the open air, and employed themſelves in hunting, carrying wood, or other exerciſes, preſerved perfect health.

We muſt conclude, that man is in a condition to ſupport a number of variations of heat and cold, between a degree far greater than that of freezing, and heat equal to, or even more intenſe, than of boiling water, which ſhows that he is not, by nature, meant to inhabit

habit certain determinate parts of the globe, but to live, multiply, and exercife his dominion in all, without experiencing an obftacle in climate. It is not fo with quadrupeds. They have been diftributed upon the earth; fome framed for warm, others for temperate, and and others for cold climates. We have not hitherto found any fpecies that can accommodate itfelf to all indifferently. The lion, elephant, tiger, panther, and leopard, dwell only in the warmer regions: when tranfported to temperate climates, they become incapable of generating, and quickly perifh in cold. Our domeftic animals do not fuffer in the warmeft regions: they cannot, however, exift in the coldeft; as, the horfe, the ox, the fheep. The elk, rein-deer, and ermine, inhabitants of the North, are never found in fouthern countries; and, fo far from being able to exift there, they do not furvive in the temperate climates. This, at leaft, has been obferved with refpect to the rein-deer, the naturalization of which has often been attempted in France and Germany; but all, inftead of multiplying, have perifhed. The law which forces animals to remain in their native countries, is under modifications. Some there are that multiply in temperate, though they are originally from warm climates; while fome animals, which are ordinarily found in cold climates,

mates, live very well in warm. The rabbit and Guinea pig are examples of the former; the beaver and lynx, of the latter.

Birds may, in this respect, be regarded as divided into two classes. Some, like quadrupeds, do not wander far from their native place, or at least do not change their climate; others have no fixed abode, but change their climate according to the seasons, being apparently necessitated to make such changes, either from the scantiness of food, or from their inability to resist the rigour of winter, or even a slight degree of cold.

We have said, with Boerhaave, that heat 149° above freezing, soon killed certain quadrupeds and birds. Indeed, 53° above blood heat, is considerable; and several species of animals cannot support it: but we do not find that it is insupportable, or that there are no animals which suffer it under the Torrid Zone, and in very warm climates. It seems to me, that we should reason upon the heat that birds and other animals can sustain, as we reason in relation to the cold; and since those of the most northern climates sustain a great degree of cold, so should the animals of the southern countries be able to support an excessive heat.

It is easy to determine, that the greatest degree of heat squamous or cetaceous fishes experience, is equal to the heat of the water

where they ſwim, which is therefore leſs than that of freezing. Thoſe living in ſalt water are expoſed to cold a little more intenſe than freezing. It thus happens, that fiſhes are ſecured againſt the rigour of cold, to which all animals are ſo much ſubjected, by the element they inhabit: and, except thoſe inhabiting ſhallow waters, they are ſheltered from the burning heat of the atmoſphere.

There are carps in warm ſprings which ſupport blood heat. I took the river carp, and heated the water where they were to 106°, without their ſeeming uneaſy: at 108, they became reſtleſs, and died at 116. I made ſimilar experiments upon eels, tench and lampreys: they died at a leſs degree of heat.

But, of all known animals, reptiles and inſects are thoſe that ſtand in greateſt dread of cold, and ſeek for heat the moſt. We may ſay, the heat of the ſun is their ſoul. When expoſed to it, they have more ſenſation and motion; and, as the heat of that luminary is more powerful, ſo are their vivacity, agility, and boldneſs greater. The venomous are then more terrible, their poiſon more dangerous. Cold produces an oppoſite effect. A number of inſects would, at the approach of winter, be expoſed to hazard, did they not ſeek a ſafe retreat in the rents of walls, the midſt of ſtones, the holes or clefts in the trunks of trees. Some ſeek for ſafet in the caverns of mountains

tains and ſubterraneous abodes, or in dung-hills, where, during the ſeverity of winter, they always experience a gentle heat. But the bottom of waters, and the bowels of the earth, are in particular two certain retreats for the greater part of reptiles and inſects. The lethargic ſlumbers they are in during winter, are the effects of the cold.

It is very poſſible, that there may be among quadrupeds and birds, perhaps even among fiſhes, ſome which experience a ſort of lethargy like reptiles and inſects. With reſpect to quadrupeds, I ſhall ſay nothing of toads, frogs, green lizards, ſmall lizards, which paſs almoſt the whole winter in the water or in the earth, in a conſtant torpor; but we obſerve the ſame facts in hedgehogs, land-tortoiſes, and alſo in ſeveral ſpecies of rats, marmots, and dormice. Thoſe animals conceal themſelves in the hollows of trees, or in the earth; ſome in ſolitude, and ſome in ſociety. Bats in winter are found ſtiff and motionleſs in the holes of trees, the rents of walls, and ſuſpended to the vaults of ſubterraneous caverns.

There are birds ſubject to the ſame torpidity. At the end of the fine weather, we ſee hundreds collect, crowd together in cluſters, and plunge into the water, where they paſs the winter in heaps, contracted within themſelves. The intelligent reader already anticipates that

I

I here ſpeak of ſwallows. The fact is too well aſcertained, too well authenticated, to leave any room for doubt. Many reſpectable and credible perſons declare, that they have not only ſeen numbers of ſwallows collect, and throw themſelves into pools at the approach of winter; but that they have ſeveral times ſeen cluſters of ſwallows taken out of water, even from beneath ice. The queſtion is, therefore, whether the ſwallows ſpoken of are the ſame with ours; that is, with thoſe that conſtruct a neſt of earth in our houſes, and remain with us during ſummer? or, whether they are ſtranger ſwallows, by which I mean a bird reſembling our ſwallow, in figure, colour, and ſize, but is, at the ſame time, of a very different ſpecies? I have, for ſeveral years, endeavoured to ſolve this. By experience I know, that the animals, which are in a ſort of lethargy during winter, are, when expoſed to a degree of cold, ſubject to the ſame effect: ſo that, if one expoſes a dormouſe, a frog, or a lizard, to the cold of freezing during ſummer, when moſt vivacious, they become motionleſs, and continue ſo while the cold remains. I thought of expoſing ſome of our ſwallows to the air of an icehouſe, making them gradually paſs from atmoſpheres leſs warm, as of a cave or a chamber adjoining to the icehouſe, as a ſudden change might be too powerful. In the month of Auguſt, they all

periſhed

perifhed in the adjoining chamber in three hours. I could not obferve, whether they had firft become lethargic, although the thermometer ftood at 43° above zero, a degree of cold rather too fmall, than too great, to produce fuch an effect. I repeated the experiment; but all had the fame fate. Whence I conclude thefe to be fpecifically different from the fwallows found in water, and below ice.

There are certainly fome fifhes, upon which the influence of cold has the fame effect. If we may credit Peclin, quoted by Haller, tench are of this kind: he fays, he has feen them bury themfelves at the bottom of the veffel upon the approach of winter, as we fee many reptiles and infects then enter the earth. But, in general, fifhes are a clafs of animals, which has the privilege of preferving vivacity and action, however cold the atmofphere may be.

Whence does it happen, that all infects and reptiles, at a certain degree of cold, lofe their ftrength, their action, and appear as if dead; while man, the greater part of quadrupeds and birds, retain their powers and livelinefs, when cold is at this degree, and even beyond it? What is the immediate caufe of the death of the former, and the prefervation of the latter, in fimilar circumftances? M. De Buffon is the firft who has ferioufly inveftigated this phenomenon. He obferves, that the animals that

become

become torpid are cold blooded, ſuch as marmots, dormice, hedgehogs, and bats; that they have not of themſelves any internal heat, but only the heat of the atmoſphere; ſo that, upon the approach of winter, their blood refrigerates with the atmoſphere. This refrigeration is the cauſe of their torpidity; and the uſe of the ſenſes and members is loſt; for it then circulates, it is probable, in the larger veſſels only. I could have wiſhed, that this explanation, ſo plauſible, was equally true. Haller, who has diſſected ſeveral hedgehogs, aſſures us he has found their blood warm; and he obſerves, that Liſter, Robinſon, and Lanciſi had before him made the ſame remark. I have made experiments upon three hedgehogs, and found their blood warm. I have found the ſame in bats, introducing, by the mouth, a ſmall thermometer into the body of the animal. M. De Buffon never ſaw the liquid riſe; on the contrary, it fell ſometimes half, and ſometimes whole degrees. But, by my experiments, the thermometer, introduced by the mouth of hedgehogs and bats, always aſcended to 99 and 101° above zero, while kept there eight or ten minutes.

It was impoſſible for me to procure marmots, but I requeſted a friend to make the ſame experiments. The conſequence was, that marmots had not cold blood, as M. De Buffon

imagined.

imagined. The heat of one marmot raiſed the thermometer from 50 to 90° above zero. The heat of another raiſed it in five minutes to 92°. Some time afterwards I had two marmots. My experiments upon them, perfectly agreed with thoſe of my friend. In the open air the thermometer ſtood at 65°; when introduced by the animal's mouth, it roſe to 101°.

There is a method of conciliating the oppoſite facts. It muſt be, that the French naturaliſt has made his experiments in winter, when the animals were devoid of ſenſation and motion. Then they would actually reſemble cold-blooded animals; the rigour of the ſeaſon had exhauſted the principle of internal heat. Reaſon, and my experiments prove, that thoſe animals do not become lethargic while the internal heat which animates them remains undiminiſhed.

It is at the ſame time certain, that the blood refrigerates in all animals that become lethargic. But may we conclude, that this lethargy is the immediate effect of the refrigeration of the blood?

Frogs, toads, tree-frogs, water-newts, I have in my experiments obſerved, after having all the blood diſcharged from the heart, or the aorta cut, leap, run, dive, ſwim; have the uſe of ſight and feeling; in a word, perform every corporal function, for ſeveral hours, the

ſame as before. I reſolved to make new experiments, and begun with frogs. I buried in ſnow ſeveral that were extremely, but equally lively: one number was untouched, another deprived of the whole blood. I even endeavoured to empty the heart and principal veſſels completely. In eight or ten minutes I took ſome from the ſnow. Thoſe which were, and were not deprived of blood, appeared preciſely in the ſame ſtate, that is, half dead, and not attempting to eſcape although at liberty. Fifteen minutes afterwards, I took others from the ſnow: all ſeemed contracted, motionleſs, and almoſt frozen. I returned them to the ſnow; and in ſome hours tranſported them to a warm ſituation, carefully obſerving what happened. By little and little, they ſtretched themſelves, opened their eyes, and prepared to eſcape. This I obſerved in all, without any difference. I had the curioſity ſtill to bury them in the ſnow. I ſaw anew the ſame phænomena; and I conſtantly found the reſults the ſame, when the experiments were repeated at different ſeaſons of the year. All the tree-frogs, toads, water-newts, whether deprived of blood or not, equally experienced the lethargic ſlumber, when expoſed to cold, but revived with a ſufficient degree of heat.

The coincidence of theſe facts conſtrains me to ſay, that the privation of ſenſation and mo-

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tion is not effected by the refrigeration of the blood, nor can we aſcribe it to the more languid circulation: whence, it is conſequent, the lethargic torpidity muſt ariſe from the ſolids alone; which, being ſeverely affected by the cold, are in a ſituation very different from the natural ſtate. What is this ſituation? It ſeems that we recogniſe it in the phænomena torpid animals exhibit: they are contracted; the muſcles have loſt their uſual flexibility; they become extended and rigid. It is therefore demonſtrated, that when the muſcular fibre becomes very rigid, this rigidity injures the irritability of the muſcles: how much muſt it be affected by a rigidity ſo great! Of this I was convinced, by irritating the fibres by different ſtimulants; but I ſcarcely occaſioned the ſlighteſt indication of rugoſity or contraction. While irritability is commonly viewed as the principle and ſource of life, when that is greatly diminiſhed by expoſition to cold, it is not ſurpriſing a lethargy ſimilar to death enſues. If this is the real and immediate cauſe of torpidity in the animals I have named, I find no reaſon to prevent its application to all animals, that in the ſame way become torpid. It is true, that it is impoſſible to deprive warm-blooded animals of what M. De Buffon imagines the cauſe of their torpidity, becauſe nature does not admit that they ſhall live without blood;

but it is always certain, that their muſcular fibre becomes rigid, and inſenſible to every ſtimulus, when in this lethargic ſlumber. This is what I have obſerved in bats, which I ſprinkled with ſalt, immerged in hot water, pricked with pointed inſtruments, and divided the pectoral muſcle with a knife. All theſe methods were ineffectual to awaken the irritability. The electric ſhock was equally fruitleſs. If cold ſuſpends the irritability of warm as well as of cold-blooded animals, and if the ceſſation of this power is ſuch, as ſeems to me the only and immediate cauſe of the lethargy of the latter, I do not ſee why it may not be the ſame with the former.

All animals that become torpid, do not ſo at the ſame degree of cold. That heat marked *temperate* upon the thermometer, ſo mild to mankind, makes dormice torpid. A degree of cold a little greater, affects bees, ſnakes, vipers, and ſome ſpecies of bats. That degree which affects toads, frogs, newts, &c. approaches freezing; but this is ſo far from affecting marmots, that 11° under zero is required to make them torpid. This variation can only ariſe from the different nature of the muſcular fibres, which ſhould render certain animals more ſuſceptible of cold than others, by ſooner producing the rigidity of the muſcles. I am well perſuaded, that impediments to the circulation,

may

may occaſion the death of many animals: but, whence comes it, that there is a great number which ceaſe not to live, although the circulation of the fluids is ſuſpended for a time, or even when abſolutely ſtopped? In another work, I have proved that ſeveral animals live a long time after loſing all their blood; that they live when the circulation is ſuſpended, by tying up the trunk of the aorta. I have remarked, that reptiles are ſubject to the ſame laws as vipers, ſnakes, eels, &c. The death of animals killed by cold, muſt then enſue from ſome other cauſe than from ſuſpending the circulation.

To diſcover the immediate cauſe of the death of animals deſtroyed by cold, I made obſervations upon ſome rendered lethargic by a degree of cold not intenſe. The phænomena attendant upon death, are theſe. The muſcular rigidity encreaſes more and more, until the fleſh hardens and freezes. The freezing firſt appears at the extremities, and gradually extends, until it reaches the centre of the animal. If the animal is then removed into a warmer atmoſphere, that the parts may relax, they will reſume their priſtine flexibility; but the animal will not revive. Its death is truly the conſequence of having been frozen. But we cannot ſay, that it happens from the freezing of the blood only: *firſt*, from all the reaſons I have

 given;

given; *secondly*, becauſe having expoſed, to the cold of freezing, animals entire and deprived of blood, all died in the ſame time. Whence I conclude, that death is occaſioned by the freezing of the ſolids. The muſcles, at a certain degree, become rigid; and, when frozen, ceaſe to be irritable. Such is the apparent cauſe of the death of animals expoſed to cold. Cold contracts the muſcular fibre, by hardening it, and conſolidating the lubricating fluid. Congelation further contributes to the deſtruction of the fibres, by turning the liquid to a number of little icicles, which, with their points and edges, lacerate the fineſt and moſt delicate parts. If the muſcular fleſh is then examined, it appears full of thoſe minute icicles; and when one attempts to twiſt or to bend it, it breaks like a friable ſubſtance.

CHAP. VI.

CERTAIN odours are to inſects the moſt virulent poiſon. Reaumur has tried upon them the effect of turpentine, and the ſmoke of tobacco. The odour of camphire, according to Menghini, and its vapour, are ſtill more efficacious when heated. I expoſed animalcula to the odour of camphire; and the reſult was preciſely

cisely the same with what has been observed of insects. The vapour occasioned inquietude and uneasiness, among the animalcula exposed. They endeavoured to retreat from the malignant odour, by retiring deep in the infusions. If the vapour was rare, they were much longer of dying than if it was dense. The odour of the oil of turpentine killed them; but the effect was less sudden than that of camphire. The smoke of tobacco became mortal in a few hours: that of sulphur killed them immediately.

Oleaginous liquids were mortal to animalcula. Corrosive and spirituous liquors killed them in a moment. Urine not only kills animalcula, but it has the property of reducing them to very small particles. As Hartsoeker observes, we could not have believed that urine, left at rest for some days, produces animalcula, if we had not remarked the same phænomenon in vinegar; although this liquid kills animalcula as readily as urine, notwithstanding that, it is full of microscopic eels. The urine, after standing some time, is covered with a pellicle, of an obscure cinereous colour, in which the animalcula are found: they are of a roundish figure, in minuteness like animated points. Urine kept for some months, has always about the same number of animalcula, but there never appears any new species: and from the ef-

fect of this urine upon other animalcula, that species must be of a nature essentially different.

We know, that the electric shock is fatal to many animals, and that it kills them the more easily as they are smaller. It requires a battery ten feet square to kill a cat or a small dog; a pigeon is killed by one only two or three feet square; and a smaller apparatus a goldfinch or a canary. I exposed animalcula to the discharge of Dr Bevis' battery. Upon this battery I put a small spot of pitch, open in the centre, and filled the hole with some drops of infusion: from this hole I drew the electric spark. Although the infusion was full of animalcula, not one survived the shock. I diminished the shock, by giving the battery a smaller charge; but the effect upon the animalcula was precisely as before. I augmented the quantity of liquid exposed to the shock, by drawing upon the spot a right line, proceeding from the central hole, two-thirds of an inch in length, and two lines in breadth. I then passed the spark across the whole fluid. This was a thunderbolt to the animalcula: all perished at the same instant. I increased the breadth of the line, without augmenting the length. So long as the breadth was only two lines, all the animalcula perished; when greater, the whole did not suffer, as they did not die till some time after. Those in the space between the two lines were stunned,

ſtunned, and revolved as if they had been carried round in a vortex: this revolution became gradually more faint, and in a quarter of an hour they remained motionleſs. Thoſe more diſtant from the formidable poſition, ſurvived longer: the moſt remote did not periſh, and their vivacity and activity ſhewed they were not incommoded by the electric fluid.

I made experiments by a ſimple ſpark drawn from a conductor. I uſed the ſame ſpot of pitch, placing it upon the conductor; and, drawing the ſpark through the central hole, it ſeemed more ſonorous and brilliant. I filled with liquid, either the central hole, or the little channel formed upon the ſpot, and varied the length and breadth. All the animalcula in the central hole periſhed; thoſe in the channel only after drawing two or three ſparks. I applied a drop of infuſion to a point upon the conductor: the animalcula died if the electric fluid paſſed through it for ſome time. In ſhort, I have obſerved, in many repeated experiments, that the weakeſt ſhock is always fatal to animalcula. Simple electricity, that is, operating ſilently, has no effect upon them. There is no kind of animalcula upon which I have not made experiments: but electricity has been alike fatal to all.

I filled ſeveral ſmall glaſs tubes with different infuſions. The tubes were cloſe at one end, and open

open at the other. I put them under the receiver of an air-pump, at the fame time keeping tubes in the open air filled with the fame infufions. For fourteen days they were deprived of air, without the animalcula fuffering injury. Upon the twentieth they begun to die, and on the twenty-fourth all had perifhed. Thofe in the open air were ftill alive. I repeated the experiment, with other infufions made of different feeds. The animalcula of fome lived a month, and even five-and-thirty days: thofe of others died in fourteen, eleven, and even in eight days: fome animalcula lived only two days.

The nature of certain animals is truly wonderful: they can perform in vacuo all the animal functions they exercife in open air. Vipers and fnakes will creep, leeches fwim; fome infects will feed, others actually copulate there. Such is alfo the nature of animalcula. They preferve every motion; they even for fome days propagate in vacuo. After a longer or a fhorter fpace, according as the animalcula are more able to fupport the vacuum, their motion relaxes, and ends with the life of the animal. It fometimes, but rarely, happens, that the animalcula will recover life upon being taken from the receiver.

I never found any animalcula in whatever animal or vegetable fubftance left to macerate

in

in vacuo: the reverſe has uniformly happened, when there remained in the receiver a quantity of air ſufficient to keep thirteen inches of mercury in equilibrio. I have obſerved the ſame phænomenon with reſpect to the eggs of inſects. I have often put thoſe of terreſtrial and aquatic inſects under the receiver of an air pump; but none ever hatched, although in every other reſpect in a condition to do ſo. The animal concentrated in the egg, enjoys the beneficial influence of the air, by means of a multitude of minute pores by which the egg is penetrated. Beſide a number of animals reſpiring by the mouth, many receive air by means of apertures in the ſides of the body, by the extremity of the abdomen, or by other parts. This is accompliſhed by many minute channels, with openings at the ſurface of the body, which, by ramifications, reach the moſt internal parts. Our animalcula, notwithſtanding their apparent ſimplicity, diſplay an organ, which we muſt imagine to be that of reſpiration. There are animalcula, that periſh the moment they are deprived of air; and there are others, whoſe nature and ſtate is ſuch, that they ſupport this privation for a longer or a ſhorter time. A ſparrow, a nightingale, a chaffinch, and, in general, other birds, periſh very ſoon in vacuo: a lizard, a frog, reptiles and inſects, ſtill longer. All the different ſpecies

cies of animalcula do not ſupport it equally well. But the loweſt rank of animalcula ſeems to be that, of all other animals upon which experiment has been made, which can live longeſt without air. None can ſupport it beyond a month; and, although they ſupport privation of air a long time, they yield under it at laſt. I well know there are inſtances cited, of different animals ſaid to have exiſted without enjoying the benefit of this element; ſuch as, the accounts of frogs found alive in the middle of the hardeſt ſubſtances; of living toads diſcovered in the centre of large ſtones, or of entire trees, where the ſmalleſt particle of air could not inſinuate itſelf into their retreats. But, thoſe hiſtories are more the object of the admiration, than the belief, of perſons who have made any progreſs in Experimental Philoſophy. It is requiſite they ſhould be ſupported by authority; which is moſt eſſential, in a caſe ſo ſtrange and paradoxical. Until we obtain facts better aſcertained, we think ourſelves entitled to aſſert, that there is in nature no known animated being, which can exiſt without enjoying the advantages preſented by air.

CHAP.

CHAP. VII.

WHILE we obſerve animalcula of ſome ſpecies, if we ſee two united together, we immediately ſuſpect they are occupied in reproducing themſelves. We have this opinion, although the animals which excite it are infinitely ſmall; becauſe many caſes ſhew, that animals in this ſituation labour abſolutely, and the moſt frequently, to propagate their ſpecies. Thus it has been thought, that animalcula copulated, becauſe they have been ſeen united in pairs. Such is the opinion of Mr Ellis, and of Father Beccaria. [a]

The tranſverſe diviſion of animalcula, not only exiſts in the ſpherical, and elliptical, but alſo in others with pointed extremities, though they have neither beak nor hook. The better to obſerve what happened, I iſolated the animal in a glaſs of liquid. If the weather is warm, the traces of a contraction are very ſoon perceived about the middle of each ſide; the contraction encreaſes, and the animalcule ſomewhat reſembles an oblong blown bladder, tied tight acroſs the middle. The contraction gradually becomes deeper, and the animal is at length changed into two minute equal ſpheres in contact in one point. Plate 1. fig. 1. A B C. The connected ſpherules continue to move as before

before the diviſion, with this difference, that they frequently ſtop, but for a very ſhort time. The anterior ſpherule ſeems heavier than the poſterior, which it drags along, the latter having no ſpontaneous motions, excepting thoſe neceſſary to ſeparate it from its companion. The diviſion is at laſt completed, and from a ſingle animalcule there ariſe two. At firſt they ſeem motionleſs; but their reſt or indolence is ſoon diſſipated, and the two portions reſume the agility of their parent whole. The ſize of the complete animal is ſoon acquired. The parts, when very near ſeparation, are not conſtantly ſpherical, but more or leſs elliptic. They are not always inactive at the moment of ſeparation, but often retain the vivacity of the whole from which they originate. What ſpecially merits obſervation, is, that in ſome animalcula, when the portions are juſt about to ſeparate, each is almoſt equal in ſize to the whole from which they originate. Of the animalcula multiplying in this way, I have counted fourteen ſpecies; but there are only two that merit deſcription. We ſee, in infuſions of red and bearded wheat, a circular animalcule above the mean ſize. Around the circumference of the body is a circle of minute protracted points, reſembling very fine cones, and moving with great quickneſs. Theſe points ſerve the animalcule to ſwim, the ſame as the limbs of ſo many aquatic animals. The animalcula

nimalcula multiply by a tranſverſe diviſion in two. The diviſion operates ſlowly; and one ſingularity is, that it is not completed till each equals the ſize of the whole, and each has during that time acquired minute points like thoſe of the old animalcule, excepting that they are ſhorter.

The other ſpecies is found in infuſions of the marſh lentil. The animalcula are ſo large, as to be viſible to the naked eye, by illuminating with the rays of the ſun a very thin-ſided glaſs tube full of the liquid. The obſerver may eaſily ſee the ſucceſſive diviſions. Other elliptic animalcula of this kind ſwim when the diviſion is ſcarcely begun, ſome when it is conſiderably advanced, and others when it is almoſt completed. In a ſhort time, a ſingle animalcule will people a whole infuſion.

There are likewiſe animalcula which multiply by a longitudinal diviſion. If a drop of infuſion is preſented to the microſcope, animalcula are ſeen among the particles; ſome are fixed to theſe by the filaments, and ſome wander at large in the drop. This filament proceeds from the poſterior part of the animal; and although its natural poſition is a ſtraight line, it often contracts and forms a ſpiral. Then the filament reſumes its original poſition. If the filament is free, it encompaſſes the animalcule. This it frequently does, almoſt periodically. It is of a pearl

pearl colour, and its ſlenderneſs extreme, at leaſt when compared with the animalcule: the length is equal to that of the animalcule, or more. The figure of the animalcule reſembles an onion; therefore I have called it the bulb animalcule. From a hole in the fore part below, proceed a number of extreme ſlender fibres, circularly diſpoſed; Pl. 1. fig. 2. Theſe fibrilli, by their conſtant vibration, occaſion a vortex in the fluid, which draws in the ſmalleſt ſurrounding bodies, and even the moſt minute animalcula. When the larger ſubſtances enter the hole of the animalcule, they are rejected, but the ſmaller remain; we have therefore reaſon to believe, that they penetrate the body of the animalcule by ſome inviſible channel, for the purpoſe of nutriment and preſervation.

I have ſaid, the filament has certain periodical motions; I may add, that the animalcule has other periodical motions, immediately ſucceeding thoſe of the filament. Always, when this contracts, the animalcule likewiſe contracts, and inſtantly draws the fibres and the hole within its body. Then it aſſumes the figure of a ſpherule, D; Pl. 1. fig. 2. It afterwards extends the filament, and becomes like a pear, E; and then aſſumes its ordinary figure, F G. The fibres and the hole re-appear: their motions re-commence with the vortices.

I

I firſt ſaw a little cleft open at the fore part of the animalcule: it was the beginning of an opening, which divided the hole in two parts. The cleft encreaſed, the vortex became double, and each of the animalcula acquired, in diviſion, the rude figure of one. The two portions ſeparated more and more; their ſhape became more perfect; and, when upon the point of ſeparation, they were metamorphoſed into two complete animalcula. One remained attached to the filament, ſoon becoming as large as the whole, and, by new diviſions, producing new beings. The other animalcule wanted the filament, rapidly traverſed the liquid, contracting and extending itſelf; and we perceived an appendage proceeding from the ſuperior part, which was the origin of the wanting filament. Fig. 3. pl. 1. ſhews the different degrees of diviſion before ſeparation.

The bulb animalcula not only inhabit boiled, but alſo unboiled infuſions of kidney beans, and ſeveral other infuſions of legumes, as lentils, beans, ſmall peaſe, grey peaſe, &c. To ſee the multiplication with facility, it is ſufficient to macerate two or three portions of each kind of ſeed in a watch-glaſs. If the experiment is made in ſummer, in a few days ſome animalcula are ſeen attached by the filaments to minute particles in the infuſion. The animalcula will divide under the obſerver's eye. The

number fixed by the filaments will be proportional to the number of divisions about to take place.

Another species of animalcule, which also multiplies by a longitudinal division, is produced by the macerated seeds. The fibrilli are not situated under the hole, but upon the lips. The figure of the animalcule resembles a monopetalous flower.

In a larger species of animalcule, sometimes found in an infusion of beets, the multiplication is effected by means of a little fragment detaching itself from the rest of the body. The figure of the body does not change like that of the rest: a small portion of the body detaches itself commonly near the part where the filament originates; Pl. 1. fig. 4. H. This fragment is in continual motion: when detached, it swims with agility in the infusion; and, although not a twelfth part of the whole, equals it in size during the day. Then it begins to multiply by the same divisions.

The reader will doubtless be curious to know, how I could isolate the animalcula. With the point of a writing pen, I transport a small drop of the infusion into a watch-glass. I put a little drop of water two or three lines distant from the first. I then make the two drops communicate by a sort of common channel, which is a prolongation of one drop with the

the pen. The animals traverſe the canal, and arrive one after another in the drop. As ſoon as I perceive one enter, I cut off the communication.

The volvox, like moſt animalcula, is very tranſparent, and the internal ſtructure is accurately ſeen. Some obſervers have already diſcovered young in the womb of the mother, extending to the fifth generation. In my long obſervations upon infuſions, I have found two abounding with the volvox; thoſe of hempſeed, and the tremella. They are alſo found in the putrid water of dunghills. Thoſe animalcula are at firſt very ſmall, but grow ſo large, as to be diſtinguiſhed by the naked eye. They are of a greeniſh yellow colour, of a globular figure, and of a tranſparent membranaceous ſubſtance. In the middle, are included ſeveral very minute globes; Fig. 5. pl. 1. Theſe minute globes, when examined with the moſt powerful magnifiers, appear ſo many ſmaller volvoces, which have each their diaphanous membrane, incloſing others ſtill leſs. I have diſtinguiſhed the third generation, but never the two others. It is poſſible they were not viſible in thoſe I examined, from their not being of the ſize or ſpecies examined by other naturaliſts. When all had quitted the mother, the common membrane burſt, and begun to diſſolve. Meanwhile, the new volvoces con-

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tained others, burst, and then dissolved. By isolating them, I saw the thirteenth generation.

One of the strongest objections made to the system of germs, arises from the great difficulty in conceiving the successive envelopement of animals in animals, and plants in plants. Oftener than once, we have found one egg within another; and some osseous part of one fœtus has been found within another fœtus. The butterfly is included in the shell of the chrysalis; and the chrysalis in the skin of the caterpillar. In the seeds of vegetables, are seen the rudiments of plants; and in the root of the hyacinth, the fourth generation has been discovered. The volvox affords a new argument for inclusion. There, we see it to the thirteenth generation; and probably that is not the last.

CHAP. VIII.

Mr Baker, in his treatise, intitled, *The Microscope made easy*, speaks of innumerable animalcula inhabiting water. He mentions a species discovered by Lewenhoek in the marsh lentil, remarkable for a long tail, which serves to moor it to the roots of this plant; likewise

for

for a cavity like a bell in the anterior part of the body, and an internal motion, which contracts and extends the animalcula and their tails at pleasure. These peculiarities are so analogous to those of my bulb animalcula, that I determined to search for the species, with which I was unacquainted.

I was observing the motions of some tadpoles about the roots of marsh lentils I had put in a vessel of water to feed them. The solar rays fell direct upon the surface of the water, shewing the roots distinctly. One was distinguished from the rest by a slight tint of shining colour surrounding it about the middle. I was surprised to see the tint disappear, and then appear, which seemed periodical. I cut away the root while the tint was visible; it immediately disappeared, and in a very short time appeared again. I examined it more narrowly, and discovered it to be a group of the tails of the animalcula I have mentioned. There were more than fifty, each fastened by the tail to a lentil root. They resembled the bulb animalcule in the faculty of extending and contracting the body and tail, in forming a vortex in the water, and in directing the swimming corpuscula to the hole or bell. The vortex is formed by fibres or minute points proceeding from the edge of the bell; Fig. 6. pl. 1. But as this species is larger, so are the

points and the vortex alſo proportionally larger. When the bell was well opened, and the animal extended, the funnel appeared to terminate in the body by a little central hole, I. They all died in a few days, without ſeeming to multiply.

Six days afterwards, I ſaw a new ſpot formed upon the roots: I ſay formed, for it certainly was not there before. This afforded new peculiarities. One portion repreſented a tree in miniature. From the trunk proceeded ſeveral branches, which divided into ſmaller branches, and theſe into others, the ſize always diminiſhing. Each of theſe laſt, at the top, bore a bell animalcule. No object could be more ſingular or more agreeable. Every three or four ſeconds, the trunk contracted towards the root of the marſh lentil where it was attached; and in the twinkling of an eye, drew in all the branches, all the twigs, and all the animalcules: but an inſtant after, the tree re-appeared in its former ſtate, with its branches and its animals.

I detached the ſhrub from the marſh lentil root, by cutting the trunk. The animals, the branches, the twigs, no longer approached the ſtem; but the ſtem, the twigs, and the branches, ſuddenly ſurrounded the animals; and, at this moment, the vortices diſappeared. Amidſt theſe alternatives, the animals, de-

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tached from their trunk, ſwam ſlowly through the water, drawing along the plant and its branches.

I viſited the plant the following day. Inſtead of ſeeing a ſingle animalcule come from the end of the branch, I ſaw two ſmaller; Fig. 7. Pl. 1. K; and theſe, as yet alone, were longitudinally marked with a very fine furrow, L. Upon examination, I found this to be the indication of a begun diviſion. They begun to divide into two animalcula; ſo that each was double. In half a day, they were ſeparated, and of their full ſize. The multiplication was very great. Two new branches were perceived to proceed from each old branch, and the reproduced animals fixed upon their ſummits. Theſe, when full grown, divided like their parents, and remained implanted upon new ſtems; ſo that the multiplication of branches was proportioned to that of animalcula; and this double multiplication was, in the ſame manner, continued for ſeveral days.

During this, the ſhrub had extended its branches in ſuch a manner, that its circumference became triple. The ſtalk and the branches encreaſed in the ſame proportion: but the death of the animalcula occaſioned that of the plant. They begun to ſeparate from the branches, as fruit ſeparates from the tree; and, as they ſeparated, the motive faculty was

loſt. No more contraction or extenſion were ſeen; the vibration of the fibres, nor the conſequent vortex: all ſemblance of animation was gone. Soon after, the figure of each animalcule was deſtroyed. The tree lived until it had loſt all its animalcules. We may ſay, that it then neither lived nor vegetated, nor exhibited any mark of ſpontaneous or internal motion.

Although the animalcula in general died where they were produced, that is, at the extremities of their branches, or, at leaſt, till the moment they are thence detached; it is not uncommon to perceive ſome ſwimming in the water, but always adhering to their ſtem, ſince we ſo term it. If, by chance, they touch a lentil root with this ſtem, they fix there, and give exiſtence to a tree, bearing as many bell animalcula as branches. The animal upon the root ſoon divides in two, then in four, in eight, ſixteen, thirty-two parts, &c. While thoſe diviſions take place, or the animals multiply, the parts of the tree multiply along with them. The number of twigs and branches ſupporting animalcula at the extremities, encreaſes. They all proceed immediately or mediately from the ſtem attached to the lentil root. This original ſtem has already encreaſed in length and thickneſs, and is truly the trunk of the microſcopic tree.

This

This species of animalcule, whose mode of production Lewenhoek could never divine, and which was unknown to Baker, is a polypus, very analogous to those called by M. Trembley, *polypes à masse.* Bonnet calls them *polypes à pennaches.* But these animalcula differ from those of M. Trembley; for the last produce the vortex, not by means of points, which they have not, but by the motion of the edges of the bell. Before the division, they lose the figure of a bell, and assume that of a round corpusculum: they are different, because they are not endowed with this alternate contraction and extension, divide unequally in two, at the division the vortex is suspended; and because the extension and contraction is not natural and periodical as in our animalcula, but occasioned by accidental influence, and is formed by the agitation of the water.

The longitudinal divisions, of which I have treated in this chapter and in the preceding, all begun at the anterior part of the animalcule, that is, the part which is before when the animalcule advances, and where, in many, the mouth is observed. But, in other animalcula, the division begins in the opposite part. One species resembles a sea hedge-hog in miniature; the figure is spherical, the surface of the body spinous, with long and pointed prickles. We distinguish the anterior from the posterior part

part of the body; for it proceeds before, and forms the vortex, by darting out the ſpines, while the other is behind: the reſt of the points are in conſtant agitation, at leaſt while the animal advances, or while it pleaſes. Another ſpecies reſembles the ſegment of a ſphere, or rather an hemiſphere, entirely covered with points. Thoſe in the concave part ſerve for fins; others form a vortex; theſe proceed from the ſection or plane of the hemiſphere, which is always the anterior part of the animalcule. Thus, both are ſeparate, and their ſeparation ſeems to ſmooth the body of the animalcule, which can remove the points at pleaſure: it even appears that its agility or ſlowneſs, and the magnitude of the vortex, is regulated by the number of points in motion. Theſe two ſpecies inhabit the tremella, and are of a coloſſal ſize, compared with other animalcula; divide longitudinally, but the diviſion begins at the poſterior part. As uſual, a faint cleft was firſt perceived, which extended as the body of the animal encreaſed, until it ſeparated into two equal parts, which, before the diviſion was finiſhed, were two complete animalcula, equalling in ſize the whole from which they came. During the diviſion the vortex continues, and the points proceed from the cleft, ſoon appearing ſimilar to the old. The completion of this diviſion requires a conſiderable time.

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In an infusion of tremella we often see two minute balls, attached by several continued points, swim rapidly through the infusion, in irregula rdirections. Fig. 8. pl. 1. M. These balls are two animalcula upon the point of division. In a moment the one ball will separate from the other, notwithstanding the apparent strong adhesion. When it is of the proper size, there is a faint constriction, and each gives birth to two minute balls, which in their turn divide again.

I have seen groups of different round corpuscula in vegetable infusions. The group is sometimes formed of four animalcula, sometimes of five or more. The corpuscula commonly differ in size, according to the diversity of the groups. Fig. 8. pl. 1. They separate one after another from the cluster, and then divide into as many portions as there were corpuscula.

I isolated one of those animated corpuscula; I confined it in a glass the moment it separated from the group. The solitary corpuscules rapidly encreased, and, when of the size of the group from which they were taken, there were seen several furrows in the body, which gradually changed to a new group similar to the old. This new group decomposed into other corpuscules or animalcula, which soon equal-

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ed in ſize and number the old cluſter decompoſed.

But the moſt ſurpriſing and the moſt extraordinary multiplication I have ſeen, is that of ſome animated globes, which roll like pellets in the infuſions of water lentil, and are viſible without the microſcope. They are externally covered with tumours, formed of ſeveral animalcula, ſituated upon each other, and attempting to eſcape; Fig. 8. Pl. 1. N. Figure a body almoſt ſpherical, formed of concentric ſtrata, each of which is an aggregate of animals. The animalcula compoſing the exterior or firſt ſtratum, ſeparate from this ſort of ſphere: then is the ſecond ſtratum laid open, which is compoſed of animalcula, and, by its ſeparating, diſcovers the third. There are even ſtrata inferior; ſo that the whole globe is decompoſed, from the circumference to the centre. The globe has no other than a rolling motion; but the compoſing animalcula have the greateſt activity. Each globe produces more than an hundred.

While the ſtrata of the globes decompoſed, I ſeized ſome animalcula, and iſolated them. At firſt, each did not equal one hundredth part of the globe in ſize; but in three or four days, every one was as large as the whole. Their motion became ſlower, in proportion as they increaſed. When full grown or complete, they

rolled

rolled with only the préceffion common to thofe globes. The furface of the ftratum was at firft fmooth; it became unequal, and loaded with tumours. Thefe tumours were fo many diftinct animalcula, which, after the feparation, fwam in the fluid. The animalcula of the fecond ftratum did the fame, likewife thofe of the fucceffive ftrata, until the globe was entirely decompofed.

Such are the modes of production among animalcula propagating by divifion. They are in reality fo many polypi, which I fhall term the Polypi of Infufions, or rather Microfcopic Polypi. Their kingdom is not bounded by the limited confines of infufions. I have at various times examined the water of ditches, dunghills, marfhes, pools, ponds, fountains, melted fnow, rain, bath and medicinal water, both of mountains and of plains, and I can affirm, that I have found them all full, more or lefs, and infinitely varied with minute polypi. The different claffes have their appointed times to originate and be deftroyed. When one fpecies of animalcula becomes very numerous, the greater part of its individuals perifh, either by difeafe or by a violent death. An infufion fwarming with animalcula to-day, will, in a few days, have almoft none. Signor Corti has obferved, that fome maintain a fierce war. We know the ingenious method practifed by that

cetaceous

cetaceous fifh, called by the northern nations, the Great whale. Having clofed up a multitude of herrings in a proper fituation, it gives the water a blow with its tail, fo as to occafion a vortex of wide extent, and great rapidity: The monfter prefents its vaft mouth and deep throat, which are foon filled with the herrings, drawn in by the vertiginous current. The carnivorous animalcula alfo form a vortex in the fluid, by means of the vibrating fibres. When fo full as to feem more corpulent, they become indolent; but if they are kept fafting in diftilled water, they become active, and employ themfelves only in devouring the fmall animalcula which are prefented. The tranfparency of their bodies permits us to fee the devoured animalcula ftill continue to move.

All the kinds of divifion I have defcribed, may be feen at all feafons, even the moft cold and ftormy. We cannot deny, that heat promotes the multiplication of animalcula, and that cold retards it; fo that we may fay, the time neceffary for divifion is nearly proportional to the heat of the atmofphere. During the rigour of winter, feveral hours are requifite; in fpring and in autumn, it is fooner performed; and in the heat of fummer, a quarter of an hour is fometimes enough for entire completion. Upon this account, the fummer in-

fufions

fusions are much more populous than the winter.

CHAP. IX.

SEVERAL kinds of animalcula are viviparous and oviparous. One of the oviparous kinds is found in rice infusions. The size is of the largest among animalcula. The figure resembles a kidney bean, except that one extremity is curved like a pointed hook or beak; Pl. 1. fig. 9. O. I isolated one of those animalcula in a watch-glass, along with a small portion of infusion, which, for security, had been boiled a long time. In seven hours, the animalcule had a companion, so like itself, that it was impossible to distinguish them. I had no reason to believe it came from without, or was produced by the infusion. Half an hour after, I visited the glass, and discovered something new; that is, two minute spheres at the bottom of the glass, one of which was oblong; Pl. 1. fig. 9. P. It moved itself several times, and changed its place. The alternate motion and rest continued an hour and a third; then became quicker, and entirely local. The sphere begun to swim slowly in the fluid; and, in a short time, the quickness of the smallest equalled

equalled that of the other two animalcula. The other ſphere, Q, included a ſpherule ſo ſmall, as to be with difficulty perceived, and would not have been noticed, but for a ſlight revolving motion upon itſelf, while the including ſphere was tranquil. In ſeveral days, the integument burſt, and the confined ſpherule eſcaped, expanded, and became ſlender at one extremity, to form the curvated beak. Theſe animalcula, therefore, ſeem to originate from an egg, repreſented by this integument. The following day, there were more than forty-five animalcula in the glaſs, ſimilar to the firſt that was iſolated. There were alſo, at the bottom, ſeveral minute balls, partly round, partly elliptic; the former leſs, the latter larger. Of the round, one after another burſt, and as many inactive ill-ſhaped animalcula came out, which ſoon acquired figure and motion. To leave no doubt, I confined ſome animalcula in a very ſmall portion of water. In ſcarcely a quarter of an hour, one was, in my view, delivered of a round corpuſcule. I ſaw more eggs laid in this manner. I counted eleven, proceeding from the poſterior part of the iſolated animalcula. Theſe gave birth to an equal number of animalcula.

Two ſpecies of viviparous animalcula are carnivorous. They ſwallow their prey by means of a great vortex drawing them to the mouth.

mouth. We ſee them diſtinctly paſs down the œſophagus, and enter a little bag, in their way to one larger, which apparently is deſigned for a ſtomach. Each animalcule has a long tail, the extremity of which is divided in two, and ſerves to fix it to the ſurrounding bodies. Upon the ſides of the tail, are ſeen two oval ſubſtances; above theſe are two ſmaller, reſembling two little narrow leaves; Pl. 1. fig. 10. R. The parts reſembling leaves, are parts of the animal; the two other, are real animalcula. If kept in view with a magnifier, we ſee them expand, quit the mother, and ſwim. The opacity of this kind of animalcule prevented my ſeeing the fœtus before excluſion; but, when it came to maturity, two ſmall ones were obſerved, where the tail originates from the abdomen. I have never diſcovered more than two in each animalcule, although I have examined many. I have indeed ſeen three attached in the ſame manner to other animalcula; but theſe I judged to be of a different ſpecies, S. Theſe two ſpecies generally inhabit the tremella of ditches.

Having taken an egg laid by an animalcule, I put it alone in a watch glaſs: afterwards, there were as many animalcula ſeen, as eggs laid by the animalcule excluded from this egg.

I iſolated viviparous animalcula, taking them before they were completely developed, and

ſtill adhering to the body of the mother. After the neceſſary time, each iſolated animalcule of the one ſpecies became parent to other two, and thoſe of the other ſpecies to three.

Theſe two genera of oviparous and viviparous animalcula, are, then, hermaphrodites in all ſtrictneſs. I ſhould conclude, that hermaphrodiſm, which at firſt ſeems confined to a few ſpecies, will extend far in the animated world.

We muſt ſay, that the original inhabitants of infuſions originate from ſome germ, or minute egg, which paſſes from the air into the infuſion, and becomes the principle or ſource of this numerous colony. This is confirmed by facts. I took the liquid from a number of eggs, ſo as to leave them perfectly dry; and thus they remained for ten days. I then put them in their native liquid, where they were ſoon hatched.

Every liquid does not equally favour the expanſion of the eggs of animalcula: pure water, for example, is moſt unfit for it. This is no more a myſtery. We conſtantly obſerve, that no animalcule almoſt ever appears in pure, or, for the greater reaſon, in diſtilled water; but I have found no fluid more favourable to the production of animalcula, than water with ſeeds infuſed, eſpecially when the ſeeds begin to become putrid.

In

In my obfervations, I have particularly inquired, whether animalcula fpecifically varied as the infufions of vegetable feeds were different, fo that each might have peculiarly its own; but I have found nothing conftant. It is true, I have often found certain fpecies of animals only, in particular kinds of vegetables, but I have frequently feen the reverfe. The animalcula of the fame infufion were different, at different times and different places; and it even is not uncommon to fee this variety in two infufions made of the feed of the fame plant. All this well agrees with the vaft variety of animalcular eggs, fcattered in the air, and falling every where, without any law. Doubtlefs, the microfcopic polypi alfo proceed from a pre-organifed principle: but is that an egg, a germ, or other fimilar corpufculum? If facts are required to folve this queftion, I ingenuoufly acknowledge, we have no certainty. The polypi die when the fluid is taken away, nor do they revive when it is reftored. It may happen, that the germs, or pre-organifed principle, are too tranfparent, or too minute, to fall under the fenfes. The idea, that animalcula come from the air, appears to me to be confirmed by undoubted facts. I took fixteen large and equal glafs vafes: four I fealed hermetically; four were ftopped with a wooden ftopper, well fitted; four with cotton; and the

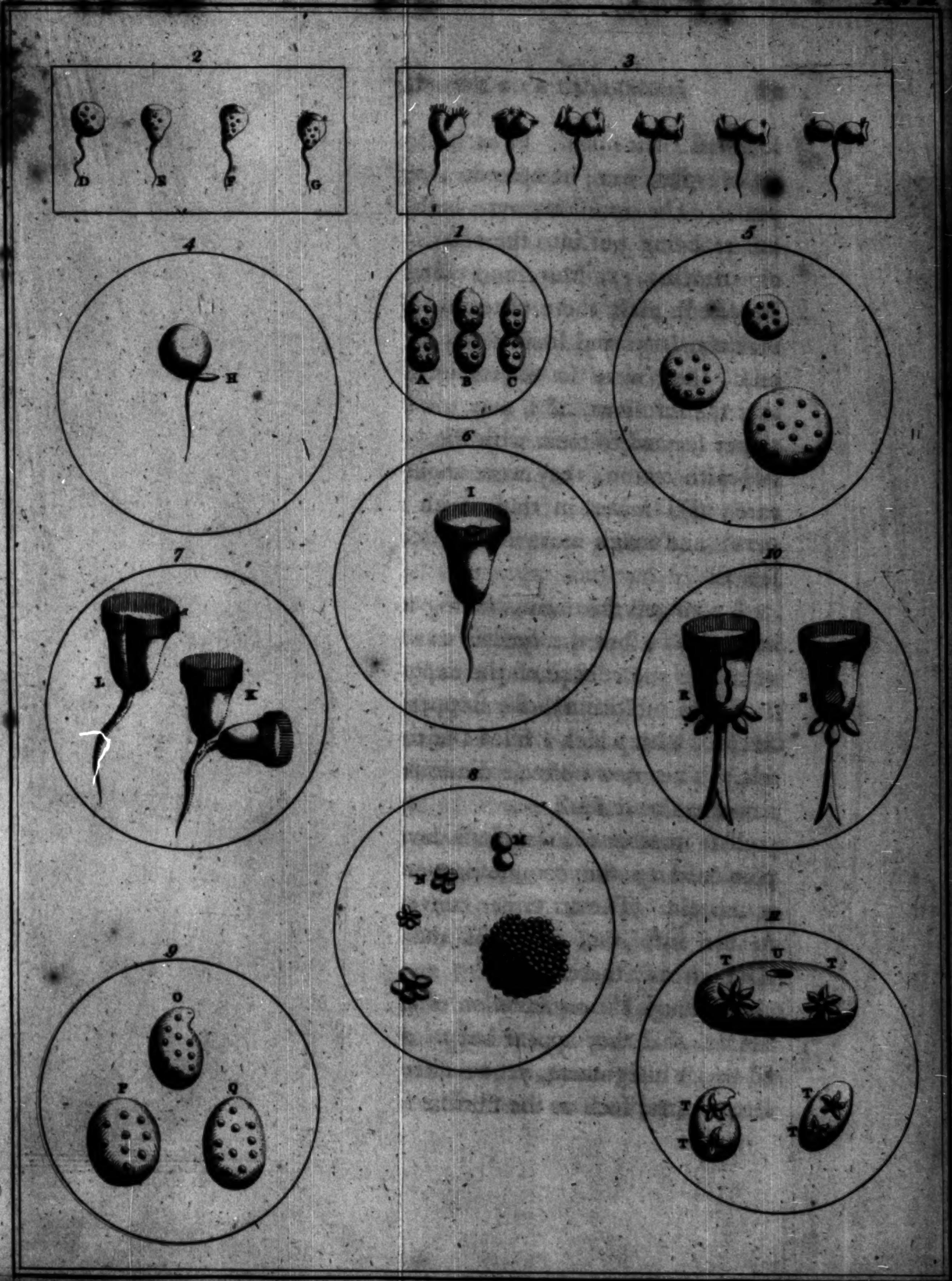
four laſt I left open. In each of the four claſſes of vaſes, were hempſeed, rice, lentils, and peaſe. The infuſions were boiled a full hour, before being put into the vaſes. I begun the experiments 11. May, and viſited the vaſes 5. June. In each there were two kinds of animalcula, large and ſmall; but in the four open ones, they were ſo numerous and confuſed, that the infuſions, if I may uſe the expreſſion, rather ſeemed to teem with life. In thoſe ſtopped with cotton, they were about a third more rare; ſtill fewer in thoſe with wooden ſtoppers; and much more ſo in thoſe hermetically ſealed.

I changed the ſeeds, taking maize, wheat, and barley; but the ſucceſs was the ſame with regard to the eſſence of the experiment.

I then ſubſtituted, for ſtoppers, nut and olive oil, with which I filled the tops of the veſſels. This new obſtacle diminiſhed the number of animalcula.

The number of animalcula developed, is proportioned to the communication with the external air. The air either conveys the germs to the infuſions, or aſſiſts the expanſion of thoſe already there.

Although the organiſation of animalcula is ſo ſimple, that they appear but as granula inveſted by an integument, yet we diſcover many different parts, ſuch as the fibrillæ for the vortex, and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and the points for ſwimming. We ſee a mouth, an œſophagus, and a ſtomach, where there is obſerved a periſtaltic motion, which puts the included aliments in motion. I ſhould mention another organ which I have diſcovered in this new courſe of obſervations; and which I ſuſpect to be deſtined for reſpiration. It conſiſts of two ſtars, with a very minute globe in the centre. They are ſituated as the foci of elliptic animalcula above the mean ſize; Pl. 1. fig. 11. T. The ſtars are always in motion, whether the animalcula move or are at reſt; but the motion is alternate and regular. Every three or four ſeconds they ſwell like a bladder, becoming three or four times larger; then they fall, and the inflation and eflation is performed very ſlowly. The ſame is ſeen in the rays, with this difference; when the globes are full, the rays are empty, or when the rays ſwell, the globes fall. During this, there is ſeen in the largeſt animalcula a very long narrow ellipſe, U, ſituated on the ſide between the two ſtars, which is in continual motion.

OBSERVATIONS AND EXPERIMENTS

UPON THE

SEMINAL VERMICULI OF MAN AND OTHER ANIMALS, WITH AN EXAMINATION OF THE CELEBRATED THEORY OF ORGANIC MOLECULES.

INTRODUCTION.

THE subject I am about to treat, should form a chapter of the preceding treatise upon animalcula, from their analogy with spermatic vermiculi; but the sound arguments of M. Bonnet have changed my determination. I sent him the results of my experiments upon Animalcula, Spermatic Vermiculi, and Mould. He honoured them with his approbation, and advised me to separate the subjects; to treat of each in a separate dissertation. In this form he thought they would more readily fix the attention, and attract the curiosity of readers. I have found his advices salutary, and have profited by them; they have enabled me to extend the subject, and enlarge my researches upon Spermatic Vermiculi.

The reality of the exiſtence of thoſe animalcula, as well as the knowledge of their peculiar nature, is a ſubject as fit to engage the inquiries of a philoſopher, as they ſeem to retreat from his penetrating examination. I may ſay, that, like the Proteus of fables, their figure and appearance change with the naturaliſts who attempt to ſtudy them. The ſeminal fluid of man, and of ſome animals, examined by Lewenhoek, appeared to him full of animalcula; which he named vermes, from their reſemblance in figure and motion. But they were ſoon conſidered by philoſophers, as a phantom of the imagination, an illuſion of the ſenſes, or ſome imperfection of the microſcope. They thought there was nothing real in what the Dutch philoſopher had deſcribed.

By others he was judged with leſs ſeverity. They agreed that there was a number of corpuſcula in the ſeminal fluid; but they denied they were animals, and thought them unorganiſed particles, which, from their ſubtility, were raiſed and evaporated ſooner than the reſt: thus, forming a fermentation and motion in the fluid, that created the idea of animation.

The celebrated Linnæus adopts this opinion nearly. He thinks the vermiculi are only inert molecules, ſwimming like oil in the ſeminal fluid, moving and darting in various directions,

 as

as they are agitated or heated by the temperature of the fluid.

Messieurs Needham and Buffon, published their sentiments upon the question, which they have elevated to the subject of the animation of those microscopic beings; and it would appear that their theories are directly opposite. The first thinks they originate from the vegetative power acting upon the seminal fluid, after it comes from the animal; by which, it is necessitated to vegetate, to expand, to put itself in motion, and to change into beings not yet animated, but simply vital.

M. De Buffon, enamoured of his organic molecules, thinks he finds them in the vermiculi; and, from a long detail of experiments and observations, endeavours to establish his theory upon the ruins of that of Lewenhoek.

Who could imagine that so many disputes, and such opposition of sentiment, would arise upon a matter of fact? I confess, this has singularly surprized me; and I have often thought that the diversity of opinion arose less from essential difficulties, than from the fault of observers, who had not the proper methods of examination; from a prejudice in favour of some theory, made their senses the cause of their errors; or, finally, from want of sufficient practice in the difficult art of accurate observation. As I treated of a subject analogous to the history of spermatic

ſpermatic vermiculi, I wiſhed to ſtudy them, to diſcover, if poſſible, where the truth was. I applied to the enquiry with all the attention, care, and reflection, in my power; and for the greater certainty, endeavoured, firſt, to forget all that had been written upon the ſubject, acting as if I had been the original author of it. In controverted facts, I have always found this the ſafeſt method to avoid confounding the opinions of the philoſopher with the reſponſes of nature. After reaping a fruitful harveſt of facts, I preſumed to think, that I knew what had been ſeen by others. I then compared their reſults with my own, and allowed myſelf to give an opinion with reſpectful deference. I doubt not but my ſincerity will be believed, when it is known that I have taken no ſide of the queſtion, and that it was abſolutely indifferent to me, whether my diſcoveries were correſponding, or contradictory to thoſe of others.

The ſeminal fluids I uſed, were that of man and different quadrupeds. I did not neglect to examine that of the ſmalleſt animals. I employed the ſeminal fluid of man as recent as poſſible; that is, taking it from dead bodies while yet warm. I took the ſemen of animals the moment they were killed. I have frequently examined the ſeminal fluid of animals alive, and ſeveral times uſed that ejected during copulation.

pulation. The importance of these facts, in the illustration of this question, will be evident to the reader of the following chapters.

LETTER FROM M. BONNET.

"In the Country, 16th October 1771.

" I agree to your request, my esteemed " correspondent, and delay not to inform you, " that I have received the excellent letter I " owe to your friendship for the Palingenesist. " Excellent book, I should say: it is almost " such in size: and I shall add it to those of " the same kind with which you have already " enriched my library. I could not resolve to " engross to myself a work, almost every line " of which demands serious attention. I de- " termined to revise it with an observer wor- " thy to understand and to follow you: I " speak of my excellent friend, the illustrious " discoverer of the polypus. Yesterday, we " perused it together; and I am unable to tell " you how much we are enchanted with it. " When you give me time, I shall detail your " observations along with you. But I ought not " to avoid remarking, how desirous M. Trem- " bley and myself are, that you should publish " separately your observations and experiments " upon Infusions, Vermiculi, and Mouldiness,

" and

" and upon the other ſubjects of the ſame na-
" ture, which you have treated with ſuch
" learning and ſucceſs. They are too import-
" ant not to merit and require a ſeparate im-
" preſſion. They would make an admirable
" appearance in detached treatiſes, and would
" thus more firmly fix the attention of ama-
" teurs. Send me them when they come
" from the preſs. M. Trembley and I will,
" under our own inſpection, cauſe them be
" tranſlated into French as ſoon as poſſible. I
" doubt not that you will comply with our re-
" queſt. This new treatiſe upon infuſions,
" will form an appendix to the former. We
" may even reprint the French tranſlation of
" the laſt, and place it at the beginning of the
" new work. Think of all this, and let me
" know. I mentioned it to M. Trembley.

" Your tracts upon infuſions, &c. will, in
" my opinion, be excellent rhetoric for the
" uſe of naturaliſts, which, I aſſure you, in
" my view, is not the leaſt merit of your learn-
" ed reſearches.

" You judged right, that I had aſſociated
" M. De Sauſſure in our amuſement; and he
" was not once abſent for fifteen days. He
" is now abſent at Lyons; but we ſhall regale
" him upon his return.

" Still a word upon infuſions.—Behold the
" poor Epigeneſiſt reduced to an impalpable
powder;

" powder; and you have no lefs pulverized
" his friend De Buffon. I have read nothing
" upon fpermatic vermiculi with fo much fatis-
" faction; and I congratulate myfelf that I
" induced you to ftudy them. Your obferva-
" tions are moft valuable, in my opinion: they
" are both new and accurate. I would reani-
" mate the worthy Lewenhoek. What plea-
" fure would it afford him, to fee the attack
" of M. De Buffon fo well repelled! I hope
" he will now be polite enough to acknow-
" ledge, his microfcopes have not done him
" juftice, and yield to your evidence.

" Your mould is almoft as new as your
" fpermatic vermiculi - - - - - But I do not ob-
" ferve that I begin to anfwer, in detail, your
" interefting letter; and, if I continued, you
" would not fo foon know that I had received it.
" I therefore end, renewing the affurance of
" my inviolable regard.

" BONNET."

CHAP.

CHAP. I.

The ſeminal fluid taken from a dead human body, reſembles coagulated milk [a]. When examined with a magnifier, the cauſe of its opacity is not diſcovered; but, when it begins to diſſolve, and aſſumes the colour of ſoapy water, if examined with a magnifier of ſmall power, the irregular parts ſeem to be in an indiſtinct, ſlow agitation. I obſerved, that thoſe parts were moved by corpuſcula infinitely more minute, of a globular figure. Each had a ſort of filament, or ſhort appendage; Pl. 2. Fig. 1. A. It is evident, that the more groſs part of the ſeminal fluid is put in motion by globular corpuſcula; for there is no motion, unleſs they are in agitation. The corpuſcula have two motions, one oſcillatory, from right to left, and from left to right, curving the appendage from one ſide to another. The other is progreſſive: the vermiculus tranſports itſelf by oſcillation. When we conſider this mode of moving, we would imagine them blind. They ſtrike againſt every obſtacle, and, when amidſt a number, make a thouſand contortions to eſcape, at laſt taking that way where they feel leaſt

[a] Dec. 21. The thermometer 48° above 0.

leaſt reſiſtance. Thus they are in continual motion. In twenty-three minutes, the motions of oſcillation and progreſſion had diminiſhed; and, in an hour and a half, it had leſſened ſo much, that a very ſmall number of corpuſcula preſerved any appearance of motion. In general, the progreſſive motion ceaſes before the oſcillatory; ſo that, at laſt, the corpuſcle merely bends from right to left, and reciprocally. They continue fixed to the ſame ſpot, until the oſcillatory motion inſenſibly dies away.

After all motion is gone, the corpuſcles remain entire in the fluid, and then they are better ſeen than even when the liquid is diluted with water. Each corpuſcle is not properly globular, but elliptic; and the appendage is not only longer than it appears, but the breadth is not equal throughout like a thread, but encreaſing as it approaches the body; Fig. 1. B. It was impoſſible to diſcern where the filament terminated, being ſo much immerſed in the fluid. When the corpuſcula have ceaſed to move, the appendage remains extended in a ſtraight line, or with little deviation.

When the ſeminal fluid has been kept for a day, or indeed for leſs, in a watch glaſs, it becomes tranſparent, although preſerving its original viſcoſity. It depoſits a ſediment of whitiſh matter, reſembling a maſs of ſlender rags.

The

The ſeminal fluid having diſſolved later than that of the preceding obſervation, I put ſome particles in rain water [a]. The ſhapeleſs corpuſcula ſwimming in the liquid, with the double motion of oſcillation and progreſſion, became motionleſs when touched by the rain water. Other waters mixed with the ſeminal fluid, produced the ſame effect as that of dunghills, rivers, ſnow, ice, and even diſtilled water. I have found nothing but ſaliva preſerve the motion of the corpuſcula. It may be uſed indifferently, either from the mouth, or when it is cold. I have often employed it to continue my obſervations. The ovular corpuſcula I then found; and the phænomena exhibited, were preciſely the ſame with thoſe of the preceding obſervation.

When the liquid drop dries up, all the corpuſcula, without exception, become motionleſs. And as it dries firſt at the circumference, that is, where it is thinner, and advances to the centre, the corpuſcula firſt becoming motionleſs, are thoſe at the edge, then thoſe of the interior part, and laſtly thoſe in the centre of the drop. If a drop of ſaliva, or of the ſeminal fluid, is put upon that which is dried, the corpuſcula do not recover motion, though the humidity continues long. The corpuſcula of this obſervation became ſooner motionleſs. In

[a] January 11. The thermometer 36° above 0.

In fourteen minutes they were languid; in three quarters of an hour there was complete repose. At last they had no progressive motion, although a remnant of the oscillatory continued; so that when the two motions ceased, most of the appendages remained extended in a straight line.

The seminal fluid taken from a dead human body, was chiefly coagulated [a]. It was then peopled by the corpuscula I have described. In one of the preceding observations, it appeared to me, that there were some larger corpuscula among the rest. I apprehended I was deceived; that this difference of size might arise from minute portions of the semen attached to the corpuscula: but I was convinced this could not be the case, because, when the seminal fluid was completely dissolved, the corpuscula retained the same size, although I passed them through another fluid; Fig. 2. C. The motion ceased two hours after the semen was taken from the body.

The seminal fluid of man was like milk ready to coagulate. I took a small portion for examination [b]. It presented a singular phænomenon. I saw four corpuscula attached by the filaments to a clot, which dissolved. They seemed to me to make every effort to disengage

[a] February 18. The thermometer 49° above 0.

[b] March 8. The thermometer 51° above 0.

gage themſelves from this incumbrance, by many motions and contortions; but they remained fixed. The filament ſometimes deſcribed a curve, and was ſometimes extended in a ſtraight line. Amidſt the ſtruggles, one extricated itſelf from the clot, and begun to ſwim in the fluid. Like the reſt, it had the motions of oſcillation and progreſſion. The other three corpuſcula did the ſame, one after another; and, by degrees, detached themſelves entirely from the clot.

The novelty of this phænomenon made me deſirous to take other molecules of ſemen not completely diſſolved, to ſee if I could find other ovular corpuſcula in the ſame ſituations. In ſome, the corpuſcula were free, and I ſaw them ſwim in the place of the clot diſſolved; others I ſaw, attached by the appendage to that part of the clot yet entire, and endeavouring to diſengage themſelves by all the contortions I have juſt mentioned. I ſaw, that when entirely detached from the ſpermatic molecules, they liberated themſelves, and ſwam about in the fluid. I found ſomething more. One of the clots was partly in filaments; many ovular corpuſcula were ſeen about the filaments, which, notwithſtanding their endeavours, were unable to ſet themſelves free. In this ſemen the corpuſcula lived two hours and a half.

When I took the ſeminal fluid from a dead human body, moſt part ſeemed diſſolved [a]. Many of the corpuſcula ſurpaſſed the common ſize. In this experiment, my object was to ſearch, with all poſſible care, for what I had ſeen in the courſe of the preceding obſervations. I ſaw all I had obſerved, excepting the phænomenon of the corpuſcula attached to clots, which could not happen here, as the ſemen was perfectly coagulated. Some of the corpuſcula continued to move for three hours.

In the examination of this ſeminal fluid [b], which was at firſt a little thick, I diſcovered, by chance, a method of ſeeing, with more convenience and preciſion, the figure of the corpuſcula and their appendages. I put a clot of ſemen upon a talc ſlider; as the thickneſs prevented my ſeeing it with accuracy, I ſwept it with a ſmall pencil, but the talc was not ſo clean, that ſome little portion of the ſpermatic matter did not remain. I cannot tell what induced me to examine the talc again with the microſcope; but I there found what I had no idea of diſcovering. I ſaw ſcores of the corpuſcula very diſtinct, although motionleſs, becauſe they were dry. They were ſeparate, without any mixture with the ſpermatic matter: the appendage of ſome was ſtraight, of others curved, and

[a] March 27. The thermometer 54° above 0.

[b] April 15. The thermometer 60° above 0.

and nearly of the ſame length in all; that is, about ſix times the length of the body, the extremity not much pointed, but thicker as it approached the body; and it was diſtinctly ſeen, that the corpuſcle made but one whole with the appendage, which, in the thickeſt part, is thrice as ſmall as the body, and even more. Each corpuſcle is ſomewhat like the red globules of blood, but ſmaller. The appendage, as the corpuſcle, ſeemed to be compoſed of a homogeneous matter; Fig. 2. D.

After this fortunate event, I frequently wiped the talc whereon I had put ſome drops of ſemen, and I conſtantly ſaw the corpuſcula with the ſame preciſion. I may add, they remained dry upon the talc, without any alteration of figure, for ſeveral days. In this ſemen they became motionleſs in two hours and a half.

I wiſhed to proceed rigorouſly with my reſearches, as it appeared the reſults I had hitherto obtained, were inſufficient to beſtow the character of real animals upon the ovular corpuſcula. We have not effectually had that congreſs of characteriſtic marks to decide their animality. Doubtleſs, we may name them moving corpuſcula, or poſſeſſing a ſpontaneous motion; for the teſtimony of our ſenſes will not permit us to ſuppoſe, this double motion of oſcillation and progreſſion, the effect of any external

ternal cauſe. The ſequel of the work will make us more perfectly acquainted with their nature.

Theſe obſervations have likewiſe demonſtrated another truth, that the duration of motion, after the corpuſcula come from the body of the animal, depends, in a certain degree, upon the temperature of the atmoſphere. At 36° above 0, all motion was gone in three quarters of an hour; at 47°, in an hour and a half; at 49°, in two hours; at 51°, in two hours and a half; at 54°, in three hours; and at 59°, only after the elapſe of three hours and a half.

From this I remarked, that the motion of the corpuſcula continued longer, as the temperature of the atmoſphere encreaſed. Examining the human ſemen in the warmer months, to learn whether the phænomena I had already witneſſed might be then obſerved, I had the pleaſure of ſeeing them again in the ſame manner. I conſtantly obſerved, that as the heat was greater, the duration of motion encreaſed; ſo that in the middle of ſummer, when the thermometer had aſcended to 81°, our corpuſcula continued to move for ſeven and three quarters, and even for eight hours.

While this heat continued, I varied the experiments. I expoſed a portion of the ſeminal fluid, taken from a man, to the air of an apartment where the thermometer ſtood at 82°; I put

put another portion in a cave, where the heat was 65°; and a third portion in an icehouse, where the thermometer stood at 42°. Here the corpuscula continued to move half an hour; in the cave, four hours; and in the apartment, eight hours. Corpuscula abounded in each drop of the fluid, and, although the drop was small, it included an innumerable multitude.

After examining the seminal fluid of man, I examined that of the horse. No method of observing it could be more convenient, as I always obtained it at the moment of copulation. I made use of the semen of different horses. The first I observed, was without clots, very fluid, and of a light cinder colour *. The corpuscula, in motion or swimming, were not so numerous as those of human semen. There was no difference in the size of human corpuscula and those of the horse, only the latter seemed a little larger. The appendage is more visible, probably because it is thicker. It is distinctly and completely seen, although immersed in the seminal lymph; Fig. 3. Their oscillatory motion is not so great as that of the human corpuscula, which may be the reason they advance further in the same time. Their progressive motion is quicker, and sometimes saltatory. The size of all is not the same, nor do all die at the same time. Some

 move

* March 11. The thermometer at 43° above 0.

move more than a quarter of an hour after the rest. The greater part died in three quarters of an hour, and sometimes they did not live above half an hour. When the motion ceases, they remain entire, with the appendages extended in a straight line, or a little curved. The semen of the horse, is very glutinous and filamentous [a]. We see corpuscula moving about, attached by the body, and in particular by the filament, to various irregular substances mixed with the fluid; and being unable to disengage themselves, the substances are sensibly agitated by their motion. More than once corpuscula are seen attached together, which might induce us to think them larger than the rest; but, with attention, the two separate appendages are soon perceived, each oscillating by itself; and if the observation is continued, it is not uncommon to see the bodies divide, and thus form two distinct corpuscula. I am well assured it is not an optical illusion, with regard to the corpuscula of a different size, but a positive fact. I may say there were some a third larger than the rest. I made this observation before, upon the 11 of March.

I made many attempts to follow the diminution of their motion. It is in proportion to the time the seminal fluid has been exposed to the

[a] March 22. The thermometer 5° above 0.

the air. Scarcely has it proceeded from the animal when the corpuſcula are ſeen in great agitation; darting forward with vaſt rapidity, and oſcillating to both ſides. This alacrity inſenſibly diminiſhes; ſo that if they at firſt deſcribe a certain given ſpace in a ſecond, in a quarter of an hour they do not traverſe a third of it in the ſame time. The arcs of oſcillation ſucceſſively become ſmaller; and at laſt the motion of the corpuſcula is reduced to a languid vibration of the body and appendage, without any change of poſition. The tremor ſoon diſappears, and the appendage remains extended in a ſtraight line, after the manner of thoſe beings.

Many aquatic animals of the apodal claſs tranſport themſelves by the contortions of their members, which vibrate and oſcillate from ſide to ſide; and indeed one may poſitively affirm, that the anterior part of the body is puſhed forward, and moves progreſſively by the contortions and oſcillations of the poſterior part. I paid the moſt ſtrict attention to ſee whether the anterior part of the corpuſcle was puſhed forward by the oſcillations of the appendage. When the motion is very rapid, it is impoſſible to obſerve it diſtinctly, from the quickneſs of the mutual vibrations of the body and appendage; but when it relaxes, we perceive with facility the mode by which they advance. And

 it

it is the same with that of the aquatic animals I have just mentioned. When the oscillation of the appendage ceases, the progressive motion of the body ceases; but the body begins to move when the oscillations of the appendage recommence. I made this important observation, not only upon the corpuscula in the seminal fluid of the horse, but upon those in the semen of all the animals that I shall afterwards name. The motion in this observation did not continue above an hour and a half.

The corpuscula in the two portions of the seminal fluid of the horse, mentioned before, were very numerous; they were rare in the last [a]. These were perfectly similar to the others both in figure and properties. Their motion continued two hours.

I examined the semen of other six horses. The corpuscula were similar to the preceding, without difference in figure or number; I therefore think it needless to stop and describe them. When the seminal fluid is mixed with water, or even with saliva, all the corpuscula instantaneously become motionless.

The seminal fluid of the bull, contains moving corpuscula, in numbers surpassing those in the seminal fluid of man [b]. The appendage is longer

[a] May 2. The thermometer 64° above 0.

[b] March 30. The thermometer 57° above 0.

longer than that of the human ſeminal corpuſcula, and the body alſo ſeems a little larger; Fig. 4. The whole length, to the extremity of the appendage, is diſtinctly ſeen, though deeply immerſed in the ſeminal matter, which is perfectly fluid, and of a whitiſh colour. The progreſſive motion is performed while the appendage oſcillates. This motion is different from that of the human corpuſcula; it is much more rapid, and ſuſpended for ſhort intervals; which is not to be ſeen in the corpuſcula of human ſemen. The ſmall quantity of the ſemen of the bull I had upon this occaſion, prevented me from extending my reſearches farther.

I was more able to ſatisfy my curioſity upon another occaſion, when I had a greater quantity[*]. Beſide the phænomena already deſcribed, I alſo remarked, 1. That the corpuſcula not only ſwam horizontally, but roſe and ſunk in the ſemen, as fiſhes do in water: 2. When the ſemen dried, the motion of the corpuſcula was irrecoverably gone: 3. In times of equal heat, the motion was quicker than of thoſe of man, or the horſe: 4. The mixture of every kind of water, and even of ſaliva, with this ſemen, was fatal to the corpuſcula.

The ſame phænomena were ſeen in the ſeminal fluid of other three bulls. I had the ſemen

[*] May 30. The thermometer 68° above 0.

men from those three at the moment of copulation.

Having opened the testicles of a dog, alive and in perfect health, the epididymis was full of semen [a]. It was a little viscous, very thick, and of a cinder colour. The thickness prevented my seeing the corpuscula accurately. I only perceived a confused agitation of the substance, which ceased upon mixing the semen with water: the corpuscula, become motionless, were distinctly seen. I then suspected, that they were the cause of the agitation, which the mixture with water had destroyed. My suspicions were confirmed upon mixing saliva with the semen; for the tumultuous motion continued. I saw it was produced by the corpuscula, the number of which was prodigious. The reader will not therefore be surprised, if I say nothing of their figure, size, motions, &c. since I would have to repeat all I have said of the corpuscula in the human semen; for the corpuscula in the semen of the dog perfectly resemble those of the human semen. They became motionless in three quarters of an hour.

The semen of a dog, I procured during the moment of copulation, was a little viscous, and like muddy water [b]. The moving corpuscula were

[a] Feb. 14. The thermometer 48° above 0.

[b] April 27. The thermometer 61 above 0.

were not ſo much immerſed, as in that taken from the epididymis; and it was unneceſſary to mix it with any other fluid, to ſee the corpuſcula. Every part was ſufficiently viſible, and their motion very rapid; but this rapidity inſenſibly diminiſhed; and, two hours after the fluid came from the animal's body, all motion was gone; the corpuſcula remaining with the appendages extended in a ſtraight line.

I repeated theſe experiments upon the ſemen of other five dogs, and found the reſults perfectly alike.

Nine hours after a ram was killed, I opened the teſticles, and expreſſed the ſeminal fluid into the glaſs of a watch [b]. All the corpuſcula were motionleſs; they were larger, and thus were more eaſily ſeen than thoſe of the dog, or of man.

Having taken the ſeminal fluid from the teſticles of a living ram, I found all the corpuſcula in motion [c]; the oval part, or body of each corpuſcle, ſometimes immerſed itſelf in the fluid, and ſometimes eſcaped the eye, then re-appearing on the ſurface. Their properties reſembled thoſe of other corpuſcula, if we add a gentle vibration and a greater activity. When the corpuſcle contracted itſelf, the appendage was leſs curved from right to left; but

[b] May 10. The thermometer 66° above 0.

[c] June. The thermometer 66° above 0.

but the total duration of motion was much ſhorter than in the others. Although the heat of the atmoſphere raiſed the thermometer to 66°, they all ceaſed to move in half an hour.

I examined the epididymis of another ram ſtill alive. The quantity of ſeminal fluid was ſo great, as to fill two thirds of a watch glaſs. Viewed with the eye, the liquid ſeemed in continual motion, although the glaſs was ſituated upon an immoveable plane. I examined a drop with a magnifier of ſmall power; the whole appeared in motion, and the microſcope proved this motion to be produced by the agitation of the corpuſcula alone. Their motion ceaſed in an hour.

After examining the ſeminal fluid of all thoſe warm-blooded animals, I thought of examining the ſemen of ſome whoſe blood is cold. I begun with fiſhes; and, for this purpoſe, delayed until they ſpawned. I took the milt of a living carp, and expreſſed the fluid into a veſſel. The fluid was tenacious, thickiſh, and of a dull white. Many moving corpuſcula were ſeen; but I could not obtain diſtinct viſion, until the denſity of the ſemen was diminiſhed by water. Then I had new objects; the corpuſcula were no longer compoſed of two parts, a body and an appendage, like the corpuſcula of other fluids; but were a united whole, reſembling minute ſpheres, and apparently ſolid; Pl. 2. fig. 5. Thoſe ſpherules,

ſpherules, of a darkiſh colour to the naked eye, ſwam through the liquid in every direction; advanced, retreated, mutually avoided each other, immerſed themſelves deep in the fluid, and ceaſed to move in a moment. In a word, they had many of the motions and properties of animalcula. The number was infinite, and they continued their courſe a quarter of an hour: then they ſtopped, and moved no more. I repeated the experiment five times, expreſſing anew the fluid of the milt, and the conſequences were the ſame. If the liquid mixed with the ſemen was freſh, ſuch as water, ſaliva, and the like, I was ſure of putting the ſpherical corpuſcula in motion, or rather of increaſing their motion. But if the mixing liquid was ardent or corroſive, inſtead of being increaſed it was deſtroyed.

At that time, I had two water newts. I cut the teſticles of a male into ſeveral pieces, and expreſſed the liquid, which was thick and glutinous. I expoſed it to the microſcope, and it changed to a maſs of long ſlender corpuſcula. Some were extended in a ſtraight line, others curved; ſome ſolitary, others entangled like a ſkein of thread. I examined thoſe alone, as the moſt eaſily diſtinguiſhed. They are not throughout of equal thickneſs. Each corpuſcle is compoſed of a body, and a very long appendage; Fig. 6. They moved with difficulty,

culty, the greater part of the body being immersed in this viscous substance. I resolved, likewise, to dilute the mass with common water; by which means, I saw the corpuscula traverse the whole liquid. As the fluid was at perfect rest, and as I saw no external cause to act upon the corpuscula, I was inclined to think this motion spontaneous, and peculiar to them. I then adopted an idea, that I had discovered the efficient cause of the motion; for, looking stedfastly, I saw two rows of minute points cover the whole appendage of each corpuscle, moving like the most minute oars; Fig. 7. During this motion, the situation of the corpuscula changed; but, when they ceased, the corpuscle also ceased to move.

When the mixture of semen and water dried up, the motion of the corpuscula was irrecoverably lost, although again wet with the liquid: but those in the mixture which did not dry, ceased to move in an hour.

I repeated similar experiments upon the fluid taken from the testicles of other newts, and I had the same results: but, upon diluting the fluid, I often saw the corpuscula collect in numbers, place themselves parallel to each other, and then bend into a circle. When all collected, they bent themselves so much, that the point of each appendage almost touched the opposite extremity of the body. In this position,

pofition, they begun to revolve around a common centre. The figure was fomewhat like a funnel. They preferved this vortical motion for fome time.

I found the corpufcula not only in the tefticles of newts, but alfo in the vafa deferentia. The veffels refemble two very white pipes: they are fituated about the middle of the vertebræ of the kidneys, one upon each fide. One end is fixed near the head of the animal; the aperture of the other is in the part through which the excrement paffes. The veffels are almoft always full of feminal fluid, but in particular while the males fecundate the eggs of the females. The femen is white like milk: the number of corpufcula it contains is fo great, that the fluid part is fmall, compared with the mafs they form. The corpufcula are perfectly fimilar to thofe in the feminal fluid of the tefticles: at the fame time with this difference, that they need neither water nor any other liquid to encreafe their motion. In the femen, they naturally move with the quicknefs of the corpufcula in the fluid from the tefticles, diluted with water.

The corpufcula of the *vafa deferentia* retain motion longer than thofe of the tefticles; and the duration of their motion equals that of the human corpufcula, thofe of the horfe, &c. I have

have always found this kind of corpuſcula in male newts at every ſeaſon of the year.

By expreſſing the ſeminal fluid from the teſticles of frogs, it may be ſeen, that it is full of corpuſcula. They are ſhorter than thoſe of the newt. They poſſeſs loco-motion as they advance. They vibrate, making gentle contortions of the body; are of a long elliptical figure, Fig. 3.; and very ſoon ceaſe to move.

CHAP. II.

After this courſe of obſervations upon the ſeminal fluid of man and different animals, I thought of reading and examining, what Lewenhoek and De Buffon had ſaid upon the ſubject, as they had made it their particular ſtudy. Several years had elapſed ſince I read their diſcoveries upon Seminal Vermiculi, ſo that only a general idea of their opinion remained. I even wiſhed to proſcribe that remembrance, and, in theſe reſearches, to have my mind as a pure tablet, the more fit to receive the real impreſſions of what my eyes might behold, without any danger of alteration, by adding any part of the obſervations of others.

I ſhall begin with Lewenhoek: and, that the reader may have before him the real ſentiments

of

of this naturaliſt, and that he may compare his obſervations with mine, I think it neceſſary to tranſcribe ſome of the chief paſſages, where he ſpeaks of Spermatic Vermiculi. Buffon has, before me, employed part of the paſſages, to compare his own obſervations with thoſe of this illuſtrious obſerver; and here I have the pleaſure of following his example. This excellent Dutch philoſopher wrote, 1677, to Lord Brounker, Preſident of the London Royal Society, to communicate his microſcopical diſcoveries upon the Human Semen.

" Poſtquam excellentiſſimus dominus pro-
" feſſor Cranen me viſitatione ſua ſæpius ho-
" norabat, litteris rogavit dominus Ham cog-
" nato ſuo, quaſdam obſervationes mearum vi-
" dendas darem. Hic dominus Ham me ſe-
" cundo inviſens ſecum in lagenula vitrea ſe-
" men viri gonorrhœa laborantis ſponte diſtil-
" latum attulit, dicens ſe poſt pauciſſimas tem-
" poris minutias (cum materia illa jam in tantum
" eſſet reſoluta, ut in fiſtula vitrea immitti poſſet)
" animalcula viva in eo obſervaſſe, quæ cauda-
" ta et ultra 24 horas non viventia judicabat.
" Idem referebat ſe animalcula obſervaſſe mor-
" tua poſt ſumptam ab ægroto terebinthinam.
" Materiam prædicatam fiſtula vitrea immiſ-
" ſam, præſente domino Ham obſervavi quaſ-
" dam in ea creaturas viventes: et poſt decur-

" sum duarum aut trium horarum eandem so-
" lus materiam observans, mortuas vidi.

" Eandem materiam (semen virile) non æ-
" groti alicujus non diuturna conservatione cor-
" ruptam, vel post aliquot momenta fluidiorem
" factam, sed sani viri statim post ejectionem, ne
" interlabentibus quidem sex arteriæ pulsibus
" sæpiuscule observavi, tantamque multitudin-
" em in ea viventium animalculorum vidi, ut
" interdum plura quam mille in magnitudine
" arenæ sese moverent: non in toto semine, sed
" in materia fluida crassiori adhærente ingentem
" illam animalculorum multitudinem observa-
" vi, in crassiori vero seminis materia quasi sine
" motu jacebant, quod inde provenire mihi i-
" maginabar, quod materia illa crassa ex tam
" variis cohæreat partibus, ut animalcula in ea
" sese movere nequirent: minora globulis san-
" guini ruborem adferentibus hæc animalcula
" erant, ut judicem millena millia arenam gran-
" diorem magnitudine non æquatura. Cor-
" pora eorum rotunda, anteriora obtusa, poste-
" riora ferme in aculeum desinentia habebant:
" cauda tenui, longitudine corporis quinquies,
" sexiesve excedente et pellucida crassitiem ve-
" ro ad vigesimam quinquam partem corporis
" habente prædita erant: adeo ut ea quoad
" figuram cum Cydaminis minoribus longam
" caudam habentibus optime comparare que-
" am: motu caudæ serpentino, aut ut anguil-

" læ

"læ in aqua natantis progrediebantur: mate-
"ria vero aliquantulum craſſiori caudam octies
"decieſque quidem evibrabant antequam lati-
"tudinem capilli procederent. Interdum mi-
"hi imaginabar me internoſcere poſſe adhuc
"varias in corpore horum animalculorum par-
"tes quia vero continuò eas videre nequibam,
"de iis tacebo [a]."

Thoſe obſervations were accompanied by others, written by Lewenhoek to the Secretary of the Royal Society, 1678. He compoſed them, becauſe ſome one had ſuggeſted to him to examine the ſeminal fluid of animals.

"Si quando canes coeunt" (Lewenhoek anſwers the Secretary) "marem a fæmina ſtatim
"ſeponas, materia quædam tenuis et æquoſa
"lympha ſcilicet ſpermatica ex pene ſolet pau-
"latim extillare: hanc materiam numeroſiſſi-
"mis animalculis repletam aliquoties vidi, e-
"orum magnitudine quæ in ſemine virili con-
"ſpiciuntur, quibus particulæ globulares ali-
"quot quinquagies majores permiſcebantur.

"A cuniculorum lymphæ ſpermaticæ gut-
"tulam unam et alteram e fæmella extillantem
"examini ſubjeci, ubi animalia prædictorum ſi-
"milia ſed longe pauciora comparuere."

In the ſame year 1678, Lewenhoek alſo communicated to the Royal Society, the animalcula he had found in the ſemen of the Dog.

H 2 "Seminis

[a] Philoſophical Tranſactions, N°. 141.

"Seminis canini tantillum miſcroſcopio ap-
"plicatum iterum contemplatus ſum in eoque
"antea deſcripta animalia numeroſiſſima con-
"ſpexi. Aqua pluvialis pari quantitate adje-
"cta iiſdem confeſtim mortem accerſit. Ejuſ-
"dem ſeminis canini portiuncula in vitreo tu-
"bulo, unciæ partem duodecimalem craſſo
"ſervata, ſex et triginta horarum ſpatio con-
"tenta animalia vita deſtituta pleraque, reli-
"qua moribunda videbantur."

Lewenhoek confirmed his diſcoveries during the following years, and made additions to them. In a letter to Mr Wren, he thus expreſſes himſelf reſpecting the ſpermatic vermiculi of Frogs.

"Hic animalculorum numerus erat tantus
"ut credere ſubiret ad quodvis femellæ ovu-
"lum a maſculo emitti forte talium 10,000 ta-
"lium animalculorum, quæ in ſemine ejus
"continentur."

In the year 1699, Lewenhoek wrote to the Royal Society of London upon his theory of Seminal Vermiculi. He believed them to be male and female.

"Si porro his addamus, quod me antehac
"in obſervationibus meis animadvertere cen-
"ſui inter animalcula ex ſemine virili quædam
"apparuiſſe, quæ aliquantulum ex ſe mutuo
"differre videbantur, unde concludere non ve-
"rebar, alterum genus mares alterum vero
"fæmellas

"fæmellas repræsentare, atque si cogitemus i-"dem in omnibus seminibus masculinis locum "habere, nullus video" - - - - -

There is another passage coinciding with this.

"Sed jam ubi in seminibus masculinis ani-"malium avium, piscium, imo insectorum, re-"peri animalcula, multo certius esse statuo, "quam antea, hominem non ex ovo, sed ex "animalculo in semine virili oriri: ac præ-"sertim cum reminiscor, me in semine mascu-"lino hominis et etiam canis vidisse duorum "generum animalcula. Hoc videns mihi ima-"ginabar, alterum genus esse masculinum al-"terum fæmininum."

In 1701, Lewenhoek wrote to the Royal Society in these words:

"Die Julii vigesima septima, circa nonam "horam vespertinam, accepi testiculos juvenis "Arietis. Cum vero lanius hisce testiculis pri-"mam detraxisset cutem seu membranam, ego "vicissim quoque eos altera privavi membrana, "ut hac ratione vasa seminifera nuda visui ex-"posita jacerent. Primo ergo aperui vasa se-"minalia in testiculi parte exteriora sita, iis-"que exemi semen masculinum (quod nudo "observatum oculo album repræsentabat colo-"rem) illudque microscopio apposui, atque hoc "pacto oculo admovi, quando mihi animalcula "seminalia tam stupendo apparuere numero,

"ut vix fidem apud quemquam, nisi testem o-
"culatum, inventurus sit. Hæc animalcula
"nubium in morem, integris agminibus, inter
"se vagabantur, natabantque, quorum mul-
"tas eodem tendere natatu videbantur, ut mox
"aliquot millena sese ab uno agmine separantia
"alteri sese agmini adjungebant, illudque sequi
"videbantur."

Lewenhoek adds:

"Hæc vero animalcula nuper a me obser-
"vata caudas habent juxta corpus crassiores,
"atque sensim fiunt tenuiores, adeo ut eorum
"extremitates ubi materia, cui animalcula in-
"sunt, atque innatant, paulo densior est, visum
"plane effugiant: atque sic horum animalcu-
"lorum caudæ fabrica plane convenit cum o-
"mnium piscium caudis."

What respects the properties of Spermatic Vermiculi, is presented in these results by Lewenhoek.

"Quotiescumque animalcula in semine mas-
"culino animalium fuerim contemplatus, at-
"tamen illa se unquam ad quietem contulisse,
"me nunquam vidisse mihi dicendum est, si
"modo sat fluidæ superesset materiæ, in qua
"sese commode movere poterant: at eadem
"in continuo manent motu, et tempora quo
"ipsis moriendum, appropinquante motus ma-
"gis, magisque deficit usque dum nullus pror-
"sus motus in illis agnoscendus sit."

From

From theſe quotations it may eaſily be ſeen, that Lewenhoek and myſelf have remarked the ſame facts in the Human ſemen. This obſerver, under the appellation of Animals, or Spermatic Vermiculi; and I, under that of Moving Corpuſcula. We both agree, 1. upon the figure aſſigned to the corpuſcula in the ſeminal fluid of man, the ram, the dog, and the rabbit. Deſcribing the corpuſcula, I have ſaid, they ſeemed compoſed of two parts, a body and an appendage. Lewenhoek alſo acknowledges the exiſtence of thoſe parts. 2. We agree concerning the ſize of the body, the length, the figure, and proportions of the appendage; which will further appear, from the deſigns he has given of the ſpermatic vermiculi of man, or the corpuſcula of which I have ſpoken. 3. We have each diſcovered a prodigious number of beings in the ſemen: we have remarked the ſize of ſome different from that of the reſt: we have allowed them the ſame properties: we have ſaid, their motion in ſwimming was ſerpentine, like that of eels; that it was uninterrupted; that towards the end of their lives, they became languid; that the vermiculi in the groſſer parts of the fluid met great oppoſition to their progreſs. But I have obſerved all this in the preceding chapter. We have both remarked, that rain water deprived the canine vermiculi of motion. I have alſo found, that

this effect was produced upon those of man, even by other kinds of water, as dunghill, ice, snow, and river water. I have constantly observed, that when motion ceases, the appendage never encircles the body, but always remains extended in a straight line, or in one very little curved. This had been remarked by Lewenhoek, as appears from his engravings of the seminal vermiculi of the dog and the rabbit. When he means to represent them dead, he exhibits the appendage extended; if he means to represent them alive, it is with the appendage curved.

The moving corpuscula found by me in the semen of man, the horse, the bull, the dog, the rabbit, the ram, newts and frogs, are therefore precisely the beings Lewenhoek terms worms, or spermatic animals. I shall likewise use this last appellation, not only to speak in the language of this naturalist, but because I esteem the facts I have related sufficient authority to bestow upon them the name of animals. The spontaneous motion, and the contortions of the body, by means of which they move from one place to another, are characteristics sufficiently decisive of their animality. Of this, we shall in the sequel have more conclusive evidence.

What I have said, is enough to shew how erroneous the opinion of Sir Charles Linnæus must

must be, when he maintains, that the spermatic vermiculi are only particles of inert matter, suspended in the fluid, and put in motion by the heat. As the celebrity of the Swedish naturalist might induce us to suppose he does not advance this without evidence, I should mention the reasons which determine him to adopt his sentiments; and, that none of their force may be lost, I shall give them, such as they have been explained by the author himself, in a Latin thesis, while he was president in December 1759, under the title, *De Generatione Ambigena.*

"Vermiculos seminales Lewenhoekii vivos "esse vermes, in omni genitura prolifica ma- "ris præsentes, ad nostra tempora firmiter "satis credidit orbis eruditus. N. D. Præses "Lugduni Batavorum 1737, commoratus cu- "riosorum quorundam amicorum, et commi- "litonum utebatur consortio quales erant, J. "F. Gronovius Floræ Virginicæ auctor, ho- "die Consul Leidensis: D. Van Svieten hodie "liber Baro, et Archiater Imperatoris: Isaac "Lawson piæ memoriæ Scotus medic. exerci- "tus Angliæ: D. Lieberkuhn P. M. Beroti- "nensis: D. Kramer auctor libri artis Doci- "masticæ: Johan. Bartsch P. M. Regiomonte "Barussus medic. Surinamensis: et D. Abrah. "Ens Pomerano, Petropolitanus. His igitur "quodam die congregatis. Ostendebat Do-

"minus

"minus Lieberkuhn præstantissima sua micro-
"scopia, quem rogabat N. D. Præses, ut ho-
"rum, ope, vermiculos in cane observandos
"præberet quod statim impetravit: contem-
"plabatur illos adcurate atque insectorum na-
"turæ gnarus statim vermiculos hosce Lewen-
"hoekianos, non esse corpora organis prædita
"et animata, atque adeo neque insecta neque
"vermes, sed particulas motas, quarum motus
"a calore dependeret liquoris rotundo ore ex-
"clamat. Præsentes omnes attenti hos intue-
"bantur et oculis suis alii credere, alii vix qui-
"dem videbantur. Conclusionem hujus rei in
"dissertatione *de sponsaliis plantarum*, anno
"1740, p. 24. edidit N. D. Præses his quidem
"verbis: *Vermiculi isti Lewenhoekiani minime
"sunt animalcula propria et voluntario motu
"gaudentia sed corpuscula inertia, quæ calidæ
"genituræ innatant; non secus ac particulæ
"oleosæ, quod selecta Lieberkuhnii microscopia
"nobis manifeste ostenderunt.* Hoc postea etiam
"vidit et confirmavit summus physiologus il-
"lustris D. V. Hallerus ut adeo auctoritas ver-
"mium seminalium jam prorsus fere in desue-
"tudinem venerit."

This singular opinion of Linnæus was unknown to me, until it was communicated in a letter from M. Bonnet, dated 20th April 1771, who transcribed the words used by Linnæus. He, undoubtedly from politeness, added the following

following paragraph, or perhaps with a design to encourage me to prosecute my researches upon spermatic vermiculi. "You perceive "there is cited here, the eminent testimony "of a Gronovius, a Van Swieten, a Lieber-"kuhn, &c. and even that of Haller. At some "future period I shall write to him about it. "Nevertheless, all those authorities could not "have weight with me; could not, in my view, "balance your opinion: because I know you "to be a much better judge of such things, "than the philosophers the author names in "his thesis. You have paid much more at-"tention to the animalcula in question, and "you have long studied the animalcula analo-"gous to them. You have thus obtained a "kind of touchstone, which experiment more "and more improves, and which can never "prove deceitful."

Before considering what was advanced by Linnæus, I could not dissemble my extreme surprise, to see Haller cited as one of those who denied the existence of spermatic animals, whereas he had always been one of their most strenuous supporters. The notes he has made upon the Lectures of Boerhaave, his Elements of Physiology, his Physiology at large; in a word, all his works bear the most manifest evidence of it. In the sequel of this treatise, I shall

ſhall have occaſion to uſe the authority of ſo great a naturaliſt.

We ſee how eaſily the opinion of the celebrated botaniſt of Upſal is eſtabliſhed. Scarcely has he viewed Lewenhoek's animalcula, when he decidedly pronounces them not to be animals. I leave it to the judgment of an impartial reader, whether a haſty glance of the vermiculi is ſufficient abſolutely to decide their nature; and to decide it better than Lewenhoek, who had, during a number of years, examined ſo many ſpecies with ſo practiſed and attentive an eye. We know well the time and labour naturaliſts have beſtowed, to be aſſured of the nature of certain organized bodies, doubted whether to be animals or plants. At the ſame time, thoſe bodies were not, like ſeminal vermiculi, microſcopic animals: their ſize admitted of their being completely manipulated, and eaſily ſeen with the eye. Linnæus ſhould have been better convinced; he who has certainly characteriſed ſo many bodies in the three kingdoms, who has made many reiterated attempts, and made them with ſo much patience. If his laborious and uſeful occupations had left him ſufficient leiſure to penetrate the world of *inviſibles*, where, as Muller obſerves, he might well be a ſtranger without a crime, and had he applied to this ſubject with that wiſdom and penetration he has diſplayed

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in the discoveries he has made upon the visible world, we cannot doubt that he would have omitted in his thesis the quotations we have just made from it. For, with an attentive view of the spermatic vermiculi, he would easily have seen, that they do not swim upon the liquid, like oleaginous particles; but that they swim at some depth. I have had eminent proofs of this. Penetrating the first stratum of the semen of the dog with the microscope, I pressed it downwards, so as to reach the lower parts: in each I saw an equal number of spermatic vermiculi.

I adopted the same method, with a considerable quantity in a watch glass. Wherever the focus of the magnifier penetrated, I saw the motion of vermiculi.

Finally, a third experiment has demonstrated their presence for a considerable thickness. I filled a thin-sided chrystal tube with semen; the calibre was half a line, the length five inches. I held it perpendicular; and, in this position, applied the lens to the sides, the great transparency of which easily permitted me to see the included semen, when the tube was interposed between the sun and the eye. Wherever I applied the lens, whether to the top, the middle, or the bottom of the tube, I always saw the vermiculi. I repeated this experiment with a tube of much greater capacity; it was

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a third of an inch in diameter within, and at least four inches long; but the opacity of the semen, occasioned by its great abundance, prevented me from seeing the vermiculi it contained. I examined the drops oozing from the lower part of the tube, which was insecurely stopped, whereby some drops escaped. All the drops were equally full of vermiculi, as the liquid in the higher part of the tube. When the experiments were repeated in this manner upon the semen of the dog, and that of other animals, the consequences were the same.

In the second place, had the Swedish naturalist bestowed upon spermatic vermiculi that attention they deserve, he must have perceived, they are not inert corpuscula; but that they have a spontaneous motion, well characterised; that this motion is regulated by the manner in which they advance; that they swim in the spermatic lymph, contorting and vibrating their members, like many other aquatic animals. He would not have ascribed the motion of the vermiculi to the heat of the semen, since, when this is gone, and there remains the heat of the atmosphere only, which happens a little after the semen comes from the animal, the vermiculi do not cease to move; but their motion continues for a given time, which is sometimes several days, when they are included in small tubes.

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In ſhort, the ſpermatic vermiculi of frogs, fiſhes, and newts, completely deſtroy the opinion of Linnæus, their ſeminal fluid being deſtitute of every ſenſible principle of internal heat. Since the blood of moſt of thoſe animals is cold, heat cannot here occaſion the motion of the vermiculi; but they ſhould be abſolutely motionleſs.

All this evinces how much two modern naturaliſts, Valmont de Bomare and Erneſt Aſch, are deceived in thinking, that ſpermatic vermiculi do not exiſt, or that they are only the moſt active parts of the ſemen; and this they maintain from their never being able to diſcover them, notwithſtanding repeated obſervations. A ſimilar objection had been ſtarted, after the diſcoveries of Lewenhoek, who was content with remitting its authors to their ſtudies, as unfit for obſervers. "Dominos illos nondum tantum "profeciſſe, ut eos recte obſervando contemplari "valeant." I muſt be pardoned if I make the ſame anſwer to thoſe new opponents; for we muſt ſay, their obſervations have been very unfortunate, or their viſion very bad, or their microſcopes good for nothing, or their unexactneſs and inexperience very great in the art of obſervation. It is true, M. De Bomare aſſerts his eyes are very good, and his microſcopes excellent; and makes no heſitation to ſay, "We "have repeated all the experiments of natural-

"iſts

"ists upon semen; and although our eyes were "very good, and our microscopes excellent, "we have been able to discover nothing." I was almost induced to exclaim, May heaven preserve such good eyes, and such excellent microscopes, for with them we shall run no risk of regarding as illusions the most beautiful discoveries philosophers have hitherto made; and we need not fear that we and our posterity will be forced to relinquish the prospect of making new ones! With this acuteness of vision, and this perfection of microscopes, we should be obliged to revisit the ignorance of our ancestors in the world of invisibles. But I would rather believe M. Bomare's sight is acute, and that his microscopes are perfect. What reason can we assign for the unsuccess of his attempts to see any thing in semen with all that assistance? The conclusion is simple. We must not say he is unaccustomed to the art of observation; I do not wish to lessen the esteem due to his merit; he has acquired the name of a laborious and indefatigable compiler. His Mineralogy and his Dictionary, which are an assemblage of fragments copied here and there, will do him this justice. But no one knows, that he has been, or that he may be, a microscopical observer; and to make microscopical observations with accuracy, many natural and acquired qualities are requisite, and many more are necessary

fary to guard againſt being deceived in the ſubtile reſearches after thoſe beings infinitely minute.

I have frequently, *viva voce*, confirmed the difference of ſentiment, that books afford concerning the nature of ſpermatic vermiculi, by means of perſons whom I cauſed obſerve the ſemen of man, and of other animals. Some, although all had diſtinguiſhed merit, were certain they ſaw nothing, although they looked long at a time through the microſcope, and even returned frequently to obſervation. The number of perſons who could ſee nothing, was not great. Others, after a very long and painful examination, ſeemed to ſee a very indiſtinct and obſcure fermentation in the fluid. A few could ſee the body, or corpuſculum of the vermiculi, but were unable to diſtinguiſh the appendage. And a very few could perceive their form and all their motions. Theſe laſt, indeed, were well accuſtomed to ſtudy microſcopical objects, and might with reaſon be termed, profeſſional obſervers. Let thoſe who deny the exiſtence of ſpermatic vermiculi, attempt to enter this laſt claſs of perſons, and practiſe upon minute objects, and I aſſure them, if they will repeat their experiments upon the ſeminal fluid, they will ſee ſpermatic vermiculi, as I, and Lewenhoek, with many other naturaliſts, have ſeen them long before me. Then, if they chooſe to

communicate their obſervations, they will have the advantage of knowing, if their publications already made excite the compaſſion of the learned, their future works will merit an eulogium.

CHAP. III.

We deſcend from the obſervations of Lewenhoek, to thoſe of M. De Buffon, which, although very numerous, comprehenſive, and ſpecific, we ſhall but abridge. M. De Buffon, with the compound microſcope, obſerved the fluid from the ſeminal veſſels of a dead human body yet warm. It was full of filaments, moving about, and branching into many parts. The filaments ſwelling, burſt, and many ovular corpuſcula eſcaped, which ſtill remained attached to the filament, as by a thread; then they oſcillated like a pendulum, and during thoſe oſcillations the thread extended. The corpuſcula, at length detached from the filaments, traverſed the moſt fluid part of the ſemen, along with their filament, the extreme length of which impeded their motion, and they ſeemed to him to endeavour to free themſelves of it. Having diluted the ſemen with rain water, the microſcopic view was better defined. It clearly appeared, that each

each ovular corpuſcle had a double motion of oſcillation, and of progreſſion. In two or three hours, the ſeminal matter acquired greater fluidity; the filaments diſappeared; the number of corpuſcula encreaſed; the threads contracted; the oſcillations relaxed; and the progreſſive motion encreaſed. In five or ſix hours, the ovular corpuſcula having loſt the threads, reſembled animals more than ever; not only becauſe their quickneſs in ſwimming was greater, but becauſe they directed their courſe to every quarter. In twelve hours, the activity of the corpuſcula was great; and ſome revolved upon their axis; others changed the ovular to a globular figure, under the obſerver's eye. Some divided aſunder; ſo that one formed two. At the end of one day, the number diminiſhed; and, upon the third, none were to be ſeen.

In other ſemen, which ſeemed entirely filamentous, the ovular corpuſcula did not proceed from the filaments; but theſe, dividing in two, were metamorphoſed into corpuſcula. They were embarraſſed by a thread. The longer it was, the more it impeded their motion; but it gradually contracted, and was at laſt completely deſtroyed. The figure of thoſe ovular corpuſcula reſembled that of thoſe of infuſions. They ſwam with a progreſſive mo-

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tion, though, at first, the thread occasioned a simple oscillation.

M. De Buffon having examined a new drop of semen, ten or twelve hours after it was taken from the animal, he saw the whole corpuscula proceed in a crowd from the same side, where there were filaments like a thread, to which many were fixed. They gradually disengaged themselves from it. The body of the thread then diminished; so that it appeared less by an half.

The author thought, in his first observation upon the human semen, that the corpuscula became gradually smaller. At the same time, he was not certain of it; but another observation convinced him.

He then observed the seminal fluid of a dog. He found it clear, and without filaments. The ovular corpuscula almost completely resembled those of the human semen; only, he found them more active, and less numerous. Upon the fourth day, very few of the corpuscula had threads.

In another portion of the seminal fluid of the same dog; beside this, he saw corpuscula proceeding from a mucilaginous substance in the semen, which, to him, seemed internally animated by an inflating motion, which induced him to think the mucus swelled in some parts to form little tumours. Those corpuscles were

were all without a thread. The figure of ſome changed. They extended, contracted, and inflated themſelves; and, amidſt thoſe wonderful operations, divided aſunder, and gave exiſtence to two corpuſcula, whoſe figure and properties were ſimilar to thoſe of the generating corpuſcula.

The French naturaliſt extended his experiments to rabbits. From the ſeminal veſſels, he took the fluid, and, mixing it with freſh water, obſerved the following phænomena: In three hours, the globular corpuſcula became ſmaller; and thus conſtantly diminiſhed until the eighth day, when they were ſcarcely viſible. But, in proportion as their ſize diminiſhed, their number and activity encreaſed: in the ſame manner their figure varied, they were ovular, ſpherical, and elliptical.

He repeated the experiment upon the ſemen of another rabbit, ejected at the moment of copulation. He found ovular corpuſcula; ſome with the filament, others without it. The former greatly reſembled thoſe of the dog, and of man; excepting that they were leſs, more agile, and the thread much ſhorter. He could not be certain that they were real threads, but only little ſpirals, formed in the liquid by the courſe of the corpuſcula.

The ſemen of the ram preſented corpuſcula moving in every direction. They were elliptical, without filaments, and equal in ſize.

M. De Buffon alſo examined the ſeminal fluid of ſome fiſhes; the carp, the pike, and the barbel; procuring it while the animal was alive. There, he found a number of corpuſcula in motion, which were of a dark colour, almoſt black, and very ſmall.

Such is an abbreviation of the obſervations made upon the ſemen of animals, by the illuſtrious author of Natural Hiſtory. And he draws a general concluſion, that the corpuſcula, examined and deſcribed by Lewenhoek near a century ago, could not, by the Dutch obſerver, or by any one, be termed Spermatic Vermiculi, becauſe they poſſeſs no characteriſtic of animality. The labour they experience in drawing along their tails; in diveſting themſelves of them; in changing their figure ſo often, to form anew under the obſerver's eye; and their diviſion into parts, and diminution of ſize: ſeem, to him, peculiarities incompatible with animality. On the other hand, not being able to ſay that they were bodies completely inert, becauſe he had really ſeen in them ſigns of animation; he inclines to conſtitute them into a particular claſs, giving them the appellation of *organic molecules*, which are particles diſſeminated through all matter, original, incorruptible, animated, and always active. Nor does he heſitate to confide the formation of the animated univerſe to thoſe molecules.

molecules. I enter not, here, into a discuſſion upon organic molecules; but, occupied ſolely with the facts M. De Buffon relates, I ſincerely acknowledge, that the eſſential differences I found between his account, and what I have myſelf ſeen, have made a deep impreſſion upon my mind. It is not, that I wiſh to flatter myſelf, my obſervations are of more value than his, from exactneſs of execution, or aſſiduity of continuance. If I may be allowed, I ſhall even ſay, that mine ſhould be preferred, as they are much more numerous. At the ſame time, M. De Buffon's perfect conviction of the truth of his obſervations; the great confidence he has, that his readers, who repeat them, will find them ſcrupulouſly exact; the liberal and natural manner in which he oppoſes them to thoſe of Lewenhoek; and the errors with which he reproaches him: all makes me judge it poſſible, that the Dutch naturaliſt and myſelf might be deceived. All thoſe things were aided by a conſideration, which, though foreign to the ſubject, is at the ſame time plauſible; I mean, the amazing celebrity and reputation, the French naturaliſt deſervedly enjoys. I long heſitated whether I ought to proſecute my obſervations, and ſubject them to as rigorous an examination as might be poſſible; or, whether it would be more proper to abandon them, leſt they might

not be credited, from the formidable trial they had to undergo. I would actually have done so, had not my illustrious friend M. Bonnet, who is well skilled in such matters, diverted my intention. He strenuously advised me to study the spermatic vermiculi of various animals. I replied, I had already done so partially; but I had suspended my labours, upon finding my observations so very different from those of M. De Buffon, whose authority I respected. He had the goodness to answer,

"You judge well, my valued correspon-
"dent, that I am not much surprised to find
"you in opposition to M. De Buffon, with
"regard to spermatic vermiculi; nor do I for-
"get what he in some measure told us, and
"which I repeat after him, that his theory
"precedes his observations. Now, you know
"as well as I do, that a favoured theory is a
"mirror which changes the appearance of ob-
"jects.

"Do not fear that the authority of M. De
"Buffon will in the least invalidate your dis-
"coveries upon spermatic vermiculi. You
"have proved yourself an excellent observer;
"you have acquired the right of being believ-
"ed. You have cherished no theory: you are
"satisfied with interrogating nature, and with
"giving the public a faithful account of her
"responses. You will always be listened to

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"by philosophers; and they will judge your "observations so much the more sure, that "you prove you possess the art of observation, "and that you neglect no rule of this art, so "little known. Proceed then, my dear Malphigi; extend your researches upon spermatic vermiculi as far as you can; and institute all the comparisons in your power between them and animalcula."

I was encouraged, by those obliging invitations, to draw my observations from the obscurity in which I had left them, and to continue to encrease them as much as my humble talents would admit. Without interruption, this was my employment during the greater part of three years. But the different facts I gradually discovered, of which I shall give an abbreviated narration, little agree with the relations of M. De Buffon: at the same time, it appeared to me, that I had, during this long research, discovered several reasons which might have induced this naturalist to think as he does.

One of the principal phænomena which the French author views as the chief basis of his hypothesis, is, what is seen in the actual formation of the spermatic vermiculi, in the mucilaginous parts of the semen and its filaments, which, under the observer's eye, change into animated beings, as he imagines he has seen

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in the semen of man and of the dog. Mr Needham readily embraces this opinion. He thinks that seminal vermiculi do not exist in the semen, while in the body of the animal, but that they are formed some minutes after it comes from the body, when it begins to change and decompose by the influence of the air [a].

In the experiments I have made upon semen, which are related in the first chapter, I have not mentioned this actual formation, because I had not seen the least indication of it. When I made observations upon semen, whether I saw the vermiculi at first, or not till the semen settled, I never perceived the gross or filamentous parts give existence to the vermiculi. It is true, in the first course of experiments, I did not think of examining this part of the subject profoundly: my attention was not fixed upon it, as it had been in my other experiments. I therefore directed my whole observations to what happened to the solid and filamentous part of the semen, at the moment of its dissolution. But I never could see the actual production of the vermiculi. I have even incontestible evidence of the contrary. In the mean time, I have discovered the paradox of the French naturalist. I wish to demonstrate this, accompanied by some facts which I must be permitted to detail.

From

[a] New Microscopical Observations.

From human ſemen, as yet but partially coagulated, I ſeparated two little clots, forming a line of filaments, which I meant to conſider with attention. The vermiculi included in the filaments occaſioned a motion; the filaments diſſolved under the eye; and the two clots, in a few ſeconds, became two drops of ſemen. It was with ſingular ſurpriſe I ſaw the ſmall number of vermiculi in theſe drops, compared with the numbers to be ſeen in others much ſmaller, not formed, as they were, by the diſſolution of the coagulated ſemen, but by a portion of the ſemen found fluid in the ſeminal veſſels.

I repeated this experiment upon a clot not ſo ſolid as the two former: there I found ſtill fewer of the vermiculi. I then begun to ſuſpect, that perhaps the vermiculi did not inhabit the groſſer parts of the ſemen, but thoſe that were fluid. This ſuſpicion was fortified, upon my ſeeing all the corpuſcula periſh when the fluid evaporated. But, that this idea might be verified or deſtroyed, I examined many clots from each ſpermatic fluid, which at firſt was very difficult to accompliſh; for the ſolid and conſiſtent parts of the human ſemen, even while in the ſeminal veſſels, are commonly immerſed in the fluid parts. I was at length able to overcome thoſe trivial obſtacles. With the extremity of a pair of ſmall pincers, I drew from

from the ſeminal veſſels a portion of human ſemen, ſimilar to milk a little coagulated: as it was moiſt, I drew it along a dry piece of glaſs, that it might depoſit its humidity. I then put it in a watch glaſs, attending its diſſolution, in order to examine it with the microſcope. It was not deſtitute of vermiculi. I made a general compariſon of the number, with the number of vermiculi in the veſſels, which had been put into the glaſs of a watch on purpoſe for this compariſon. But there was no proportion; the number in the ſolid parts was ſo much ſmaller than in the fluid parts.

Theſe reſults did not ſatisfy me. I ſaw that the few vermiculi in the ſolid ſemen, might be owing to ſome little portion of the fluid remaining in the ſolid piece. At the ſame time, inſtead of finding few, I could have wiſhed to find none at all, or at leaſt a very ſmall number. Having taken another clot of human ſemen from the veſſels, I endeavoured to diſengage it as far as poſſible from the ſeminal fluid that might remain about and within it. Here I ſhould obſerve, *en paſſant*, that this operation of drawing along the ſolid part to take away the fluid, ſhould be performed with great adroitneſs and celerity, otherwiſe the ſolid part gets time to diſſolve during expoſition to the air. And it then happens that drawing it long,

or

or ſlowly, along the glaſs, inſtead of drying, it always becomes more humid. I ſucceeded, at laſt, in freeing a clot of human ſemen from all ſenſible moiſture. I put it in a watch glaſs, and, when diſſolved, examined it with the microſcope. Truth obliges me to declare, that here I found no vermiculi; nor did I find any in other clots managed after the ſame manner, although they were numerous in the fluid parts of the ſemen in the ſeminal veſſels of the animal which had afforded me the clots and the ſemen. I did not delay a repetition of this important obſervation. In my journal of experiments, I find this has been done fourteen times, in thirteen of which the diſſolved clots exhibited no vermiculi; and only once I found a ſmall number in the coagulated clot.

The ſeminal fluid of the rabbit is always partially coagulated; ſo that whenever taken from the veſſels, it afforded me the means of repeating my experiments. But I have never diſcovered vermiculi, when I was able to take the ſpermatic lymph completely away. Theſe united facts convince me, that the habitation of the vermiculi is in the fluid part of the ſemen. The ſame facts aſcertain the degree of credibility we ſhould beſtow upon what M. De Buffon ſays of their formation. It is evident, that far from being generated by the ſolid or filamentous part of the ſemen, from which

they

they detach themſelves, they do not branch out as I have demonſtrated: if they are even found there, they proceed from the fluid part, which is the place of their natural abode, and which is at this moment mixed with the ſolid. It is doubtleſs this which occaſions the error of M. De Buffon. He ſaw the moſt groſs and filamentous part agitated and moving: amidſt its agitations and contortions he ſaw vermiculi proceed from it: he even obſerved the number encreaſe, proportionally as the bulk of the groſs and filamentous part decreaſed: he likewiſe remarked, that the number was greateſt when the filaments had entirely diſappeared; and, allowing himſelf to be deceived by theſe appearances, it was eaſy to believe the decompoſition of the filaments was the productive cauſe. But the fact is, the vermiculi pre-exiſted in the filaments; they were concealed and enveloped in the parts immerſed in the ſeminal fluid; and, only when diſengaged, do they become viſible to the obſerver: nearly in the ſame manner, as if one had ſteeped a piece of ice in an infuſion full of animalcula, and taking it out it was carried to the fire; as it melted, it would exhibit the animalcula that had inſinuated themſelves into the crevices.

M. De Buffon, by a very ſimple experiment, might have ſatisfied himſelf, that the vermiculi exiſted

existed before the dissolution of the semen. He should first have examined the fluid part of the semen, which, upon opening the seminal vessels, we find mixed with the solid. There he would have found abundance of vermiculi, although at that time they could not be produced by the dissolution of the solid semen; since we see that this solid part begins to dissolve, only when it proceeds from the animal, and experiences the influence of the air.

What I have already said, and what I shall continue to say, proves the falsity of Mr Needham's opinion, who assures us, the vermiculi are produced some minutes after the semen comes from the animal's body; that is, when it begins to be altered and decomposed by the action of the air. Respecting the semen of man, it is necessary to consider whether the portion is solid or fluid. If the former, when deprived completely of the spermatic fluid, no vermicule is ever seen, although it remains during some time exposed to the air, and although it changes and is decomposed. If the latter, the vermiculi appear in it before the time required for this alteration. It has often happened, that the time I consumed in taking the fluid matter from the seminal vessels still warm, to present it to the microscope, did not exceed a second; yet I found the same number of vermiculi as afterwards, even when the fluid had been

been exposed to the air sufficient time to effect its decomposition.

The observations I have made upon the semen of other animals, further convinces me of the falsity of such an opinion. I prepared a ram, alive, and vigorous; so that a friend cut away the epididymes, which are commonly full of semen; and while he cut them, one might take a drop of semen, and immediately present it to the microscope, where I kept the eye ready to observe. It may easily be seen, that the time in performing this operation could not be shorter. We saw numbers of vermiculi, and all were very vivacious. I repeated this experiment upon the semen of the newt, and I saw the same. Further, in the spring, when the *vasa deferentia* are full of semen, it was unnecessary to open the animal, to obtain it for observation. Upon pressing the belly gently, it escaped by the anus, where the two vessels terminate.

This animal has afforded me a proof still more decisive. I let the males suffer hunger so long, that they became very emaciated; then the *vasa deferentia* contained a very small quantity of semen, and, from the transparency of the tunics of the vessels, which were rendered thin, it might easily be observed with a magnifier. Opening the abdomen, and applying the microscope to the vessels, without affecting or deranging

deranging their situation, we saw the vermiculi swimming in the fluid; and they were visible to those who were least skilled in the art of observation, because, as I have remarked in the first chapter, they were much longer than others.

I may therefore conclude, that the spermatic vermiculi of man, and of animals, exist in the semen, previous to any alteration or decomposition by the influence of the air; and that they are active in this fluid, even while it is included in the organs of generation.

CHAP. IV.

I CONTINUE to examine the other principal results, from the observations of M. De Buffon. In this number I place the appearance of those very long and slender filaments, the vermiculi drag along in their course; the contraction and disappearance of the filaments; the activity the vermiculi acquire with age; the facility of motion in every direction; the change of form, division, diminution of size; and, lastly, their total destruction in a few days.

To begin with the first, I may observe, that Lewenhoek and other naturalists have remarked, that each vermicule in the semen of man,

or of ſeveral other animals, has a long appendage, which it drags along with it as it ſwims. But this appendage is not ſuch as M. De Buffon would incline; he imagines it a thread or long corpuſculum, having no analogy to a tail, or any other member, and entirely foreign to the ſpermatic vermicule. I have always found, that the appendage, as I have ſhewn in my firſt chapter, has every characteriſtic of an actual tail. It has the ſhape; and the diameter always becomes greater, as it approaches the body of the vermicule to which it is united. So that it clearly appears to form one whole with it, as is ſeen in the rat-tailed worm: ſo much the more, as, in ſwimming through the ſpermatic fluid, the vermicule makes uſe of the tail. In effect, it bends it firſt to one ſide, then to another, and in every different direction, as aquatic worms are wont to do while they ſwim. I have often ſeen this, and in the moſt diſtinct manner. So that I muſt diſcredit my eyes, if I thought or wrote differently.

It is true, that this microſcopic obſervation upon the tail of human ſpermatic vermiculi, is the moſt nice and delicate of any I have made; it demands the greateſt care, and the moſt ſtrict attention. The tail is wonderfully ſlender, and at the ſame time tranſparent; whence it happens, that too ſtrong a light confounds it with the ſeminal fluid, ſo that we entirely loſe ſight

of

of it. The choice of the light fit for obſervation, is then of the greateſt importance. The direct light of the ſun is too powerful, as is that of a lamp; at leaſt, unleſs it is by ſome method moderated. The light I have found moſt apt for this fine obſervation, is that of a window oppoſite a white wall, moderately illumined; as, for example, when it is expoſed to a part of the ſky covered with white clouds.

In the ſecond place, the finer and thinner the ſliders are, on which the drop of ſemen is depoſited, the more eaſily are the tails diſcovered. I prefer talc to chryſtal.

Thirdly, the ſpermatic drop ſhould be as thin as poſſible, otherwiſe the origin of the tail will not be ſeen, the reſt is concealed in the liquid: and, even when the ſeminal fluid is troubled, it is neceſſary to refine it with another portion more clear.

Fourthly, when the vermiculi ſwim, as the tail is always a little lower than the body, we muſt depreſs the focus of the lens a little, to obſerve it.

Finally, a microſcope of a ſingle lens, ſuch as that called Lewenhoek's, ſhould, for this obſervation, be preferred to the compound microſcope.

Although M. De Buffon mentions, in his obſervations, only one or two of the precautions alluded to, and which he ſeems to have

used, yet I am not willing to think he has neglected the rest; for the exception of only one, would prevent him from making the real observation. He says, that he always used the compound microscope, to examine the semen of man and animals. I doubt not, his microscope was as excellent as he says; but it was a compound one, and had all the defects of compound microscopes, among which this is specially placed: the object observed is never seen so distinct, or its outlines so well defined, as with a perfect microscope formed of a single lens. This fact is acknowledged by all observers; and the observations I have made upon human spermatic vermiculi, with Lewenhoek's microscope, confirm it more. Using the first, I saw the vermiculi precisely as I have described them; and with the other, I had a confused view of the body, which I frequently doubted to be round or oval: the outline was always in a sort of mist or cloud. The tail, which is infinitely more delicate, appeared less sensible; and I only knew to distinguish it as a long slender body. It is not surprising, therefore, that M. De Buffon calls this part of the vermiculus, a foreign body, a sort of long, delicate, and subtile filament, since to him it appeared truly such, when viewed with the compound microscope.

But

But it may perhaps be thought, that the compound microſcope uſed by me was not ſo good as M. De Buffon's. However, I can aſſert, that my microſcopes are the moſt perfect now made in London. I ſhall alſo add, that I was deſirous to view ſpermatic vermiculi with the microſcope M. De Buffon has uſed, that is, Cuff's compound microſcope, which was preciſely that of Mr Needham, and which, M. De Buffon ſays, he uſed in his obſervations upon the ſeminal fluid. But it ſhowed me no more; and I may ſay, that my obſervations and experiments, as well upon ſpermatic vermiculi, as upon the animalcula of infuſions and other ſimilar beings, could never have been exempted from contempt, I will even ſay error, if I had preferred the compound microſcope to that of Lewenhoek.

Let us ſtop a moment, to eſtabliſh the certainty of the exiſtence of tails in the human ſpermatic vermiculi. The fact is confirmed in ſo convincing a manner by the experiment upon talc, where the tails appear complete, and not confounded with the ſemen, that this experiment would put it beyond a doubt, was there no other proof. In the mean time, we may obſerve, that ſuch extreme circumſpection and ſo many precautions, are not neceſſary to diſtinguiſh the tail of the vermiculi of many

animals, when Lewenhoek's microſcope is uſed for examination.

Reſpecting the contraction and entire diſappearance of the tails, which M. De Buffon ſays he obſerved, when the vermiculi remained long in the ſemen after it came from the animal's body; I can oppoſe nothing, except that, in my numerous obſervations, I always ſaw the contrary. The vermiculi conſtantly preſerve this member, not only while alive, but alſo after death, and even long after death; ſo that it only begins to ſpoil, and be deſtroyed, when the vermiculi themſelves arrive at this ſtate. I ſhall ſay more: the maceration of the dead bodies of the vermiculi produced by boiling or infuſion, is not ſufficient to deſtroy their ſtructure or their form. Vinegar, nay urine, fluids which entirely deſtroy the contexture of moſt animalcula, cannot, until after a conſiderable time, affect the body or the tail of our vermiculi. It would therefore be neceſſary to deny the evidence of all theſe facts, to agree with M. De Buffon concerning the contracting and diſappearing of the tails of ſeminal vermiculi. That my obſervations may have greater weight, to eſtabliſh the certainty and continuance of thoſe tails, I ſhall cite the authority of the great Phyſiologiſt of Bern—" Nunc quod caudas attinet perpetuam particulam vermiculi " ſpermatici, ea nimis certos et fide digniſſimos

" teſtes

"testes habent, quibus meum suffragium per "experimenta natum addidisse liceat [a]."

The other phænomena of spermatic vermiculi observed by M. De Buffon, such as, an increase of action they acquired, with time, their change of shape, their diminution, their division, and the duration of their lives from four to eight days, seem to me no less paradoxical, if we attend to the repeated observations mentioned in the first chapter, and if we recal those of the celebrated Lewenhoek. The acknowledged merit of M. De Buffon, prevents me, at the same time, from considering this collection of facts as chimerical: but, not being able entirely to reject his singular microscopical observations, however defective his microscope might be, it could not occasion such a difference of phænomena. I therefore determined to resolve his doubts, by taking the trouble to make a new course of experiments upon the human semen, and also upon that of other animals: but, notwithstanding all my care, precaution, and all my possible vigilance, I could discover nothing new, at least essentially affecting the facts I have spoken of. But, with reflection upon M. De Buffon's observations, and the repetition of my own, I cannot reproach him with seeing what did not exist. I thought that all this might be an equivocal effect,

[a] Haller. Physiolog. tom. 7.

effect, which seemed to me the more likely, as the phænomena he says he observed in spermatic vermiculi, might be occasioned by beings of a very different nature. My experiments upon infusions, suggested this idea. I had remarked, that there is no part of an animal which, when infused, does not give existence to a particular kind of animalcula. They are produced indifferently, by the muscles, brain, nerves, membranes, tendons, veins and arteries. The same is done by the blood, serum, milk, chyle, saliva, &c. mixed with water, or even by themselves. I had not yet made experiments upon the human semen for this purpose; but it was most probable, that the putrefaction of this liquid would give existence to particular beings:—and who knows, said I to myself, that they have not inadvertently been confounded with seminal vermiculi, and that M. De Buffon has not ascribed to the latter the properties and phænomena exhibited by the former? When I again perused Haller's Physiology, I found that he was of this opinion, and that he even suspected, M. De Buffon had never seen the spermatic vermiculi.—" Porro Buffonius, ut " cum illustris viri venia dicam, omnino non " videtur vermiculos seminales vidisse. Diu- " turnitas enim vitæ quam suis corpusculis tri- " buit, ostendit non esse nostra animalcula, id " est spermatica, quibus brevis et paucarum ho-

" rarum

"rarum vita eſt." And in another part of the ſame volume he adds—"Ea experimenta, "id eſt Buffonii, manifeſte ducunt ad animalia "putredinoſa." However, I thought of aſcertaining this fact, by obſerving what ſucceſſively happened to the ſemen when long preſerved in a watch glaſs.

I made my firſt experiments upon the human ſeminal fluid [a]. The vermiculi died in three hours and a half, and were precipitated to the bottom of the glaſs. Upon the ſixth day, the ſeminal fluid begun to exhale a fœtid and diſagreeable odour; but I could perceive no animated being; only, upon taking ſome drops from the bottom of the glaſs, I obſerved the dead bodies of the vermiculi apparently very entire. The ſeventh and eighth days, I ſaw no change; but the fœtor encreaſed. On the ninth, I diſcovered very minute animalcula, their ſize nearly equalling that of ſpermatic vermiculi; but they had no tail, and greatly reſembled moſt minute ſpherules. Like the animalcula of infuſions, they ſometimes ſtopped at little fragments of corrupted ſemen: ſometimes their courſe was very rapid, retrograde, riſing and ſinking in the fluid: in a word, they poſſeſſed every property of infuſion animalcula. They were ſeen in every ſtratum of the ſemen; and thoſe at the bottom, put in motion the dead bodies

[a] February 11. The thermometer 45° above 0.

bodies of the ſeminal vermiculi, which were ſtill entire, and remained ſo ſome days longer. In the ſeminal animalcula, we remarked the ſame terms, with regard to the encreaſe and diminution of the numbers, and their end, which we generally remark in the hiſtory of other animalcula; only, when the globular animalcula decreaſed, there came other two generations: the laſt of which could ſcarcely be diſcerned with the naked eye, and, in ſome obſervations, remained until the eighteenth day. While examining the ſemen in this glaſs, I obſerved a portion of the ſame kind which I had put in another glaſs, and placed it in a ſtove, that it might experience a greater degree of heat. The effect of this temperature was ſuch as we might expect. The vermiculi lived longer, and the infuſion animalcula appeared ſooner. The vermiculi died in five hours, and the animalcula were ſeen in five days. They were of a globular figure, had no tail, and all were of the ſame ſize and ſpecies as thoſe of the preceding obſervation. The other two colonies of ſmaller animalcula then appeared; the globular always remaining.

Having again prepared the ſame kind of ſemen [a], I had an opportunity of ſeeing the effect of heat in accelerating the production of animalcula. I found ſome in the ſeminal fluid, in

[a] May 22. The thermometer 65° above 0.

in lefs than twenty-three hours after it was taken from the dead body. The fpecies were different from the globular; they were three times as large, and of a cylindrical fhape. When fwimming, the body undulated like a ferpent; which did not occur, or was not obferved, in the globular animalcula. The dead vermiculi appeared to be their food; fince the animalcula, inceffantly in motion, furrounded them, and pecked at them with the anterior part of the body. In three days I faw other animalcula, as minute as the fpermatic vermiculi, along with the cylindrical animalcula; and I remarked a circumftance, which in femen I had not obferved before. In the treatife upon the animalcula of infufions, I have fpoken at large of a propagation by a natural divifion, which happens with many fpecies. I have faid, that, in feveral, the divifion begins in fuch a manner, that the animalcula is gradually cleft afunder, until it is divided into two equal parts, which become two animals fmaller than the firft. But it remained to enquire, whether this mode of propagation could happen with animalcula fimilar in fize to feminal vermiculi. More than ten divided tranfverfely before my eyes; and this divifion, or propagation, continuing feveral days, the feminal fluid, which had become exceffively fœtid, now teemed with life: but the number of thefe, as well as of the cylindrical

cylindrical animalcula, gradually diminiſhed, in the ſame way as the numbers of infuſion animalcula decreaſe; and upon the twenty-firſt day all had diſappeared. There only remained in the ſemen, a univerſal obſcure fermentation, in no particular direction: but the ſeminal molecules were tumultuouſly driven about in every direction. I was not long of perceiving, that this irregular agitation was occaſioned by a multitude of the moſt minute animalcula, concealed amongſt the ſemen, which, in their courſe, they put in motion: and of this I had complete evidence, by diluting the ſemen with water, as they were then accurately diſcerned, appearing about half the ſize of the ſpermatic vermiculi.

The phænomena, diſcovered in the ſemen of the horſe, were analogous to thoſe I found in the human ſemen. The vermiculi lived ſeven hours, and then were precipitated to the bottom of the ſemen, where they long remained entire. The body and tail were complete for a month. In fourteen hours the ſemen began to exhibit ſymptoms of putridity, and then were ſeen the animalcula of infuſions: they increaſed; and, at the beginning of the fifth day, there were many ſpecies. One particularly demands our notice. Not only did it multiply by a longitudinal diviſion, but the figure and ſize of the animalcule changed every moment.

Sometimes

Sometimes the animalcule contracted, and became round; sometimes it dilated, and became elliptical; as I have remarked in several species of infusion animalcula.

When this experiment was made upon the semen of the horse, I made one, similar, upon that of the rabbit. The vermiculi perished and fell to the bottom in four hours; the animalcula of infusions appeared only after fifteen. Two species multiplied by division, and one exhibited the contractions and inflations I mentioned before.

I made the same experiments upon the seminal fluid of the ram, the dog, the bull; upon that of frogs and newts. The results were constantly analogous. At the beginning, and during the progress of putrefaction, each produced animalcula; which occasioned our remarking, in all that appeared, a number of various circumstances. Their number encreased, it diminished, and became very small. The animalcula were different in figure and size; and some species multiplied by division: which evinced, that animal semen resembled infused vegetable seeds, from the many kinds of beings to which it gave existence.

These facts afforded new light to illustrate a truth, sufficient to satisfy me, how erroneously M. De Buffon had ascribed to the seminal vermiculi, properties belonging only to the animalcula

animalcula of infuſions. Let us, in a few words, collect the circumſtances. According to this author, after a certain time, the vermiculi were deprived of their tails. He ſhould have ſaid, that the animalcula of infuſions came in the place of the vermiculi which were already dead, and precipitated to the bottom of the liquid. He was arreſted by their firſt appearance, and took them for ſeminal vermiculi deprived of the tail, which, in truth, they often very much reſemble. When diſengaged from the tail, M. De Buffon adds, they acquired greater activity. This was a neceſſary conſequence of the firſt miſtake. When the infuſion animalcula had taken place of the vermiculi, their encreaſed agility could not be unobſerved, ſince the animalcula move with much greater quickneſs than the vermiculi. This erroneous ſuppoſition being admitted, M. De Buffon had to relate, as he has done, the remainder of the phænomenon. He had to ſpeak of the imaginary changes of the vermiculi, of their diviſion and their diminution, with the more confidence, as his opinions had to be confirmed by a repetition of his experiments, if not upon all, at leaſt upon many ſpecies of infuſion animalcula in the ſemen.

I think I have had too numerous, and too deciſive proofs, to doubt that the phænomena ſeen in infuſion animalcula of ſemen, are different

ferent from the phænomena exhibited by the seminal vermiculi. Will I not be permitted at once to oppose my experiments to those of the illustrious M. De Buffon? I see that all the seminal fluids he has studied, and where he has discovered the phænomena of which I have spoken, have also been studied by me. I perceive that I have even examined more, both of cold, and of warm-blooded animals. His observations upon the fluids, were only made at certain times, and in one season. I esteemed it necessary to extend my observations to all seasons. My microscopes were not inferior to his, as I have said; nay, I may say they were much better. How then is it possible, that, being in the same circumstances with the French naturalist, and being even in situations more advantageous, for observing the phænomena of spermatic vermiculi; how is it possible, I say, that I have never observed those phænomena, that I have never observed some, or remarked but a single one? What do I say? Not only have I never observed any of those phænomena; but I have seen what completely contradicts M. De Buffon's observations, and remarked it in all the fluids I have many, many times examined. Such, for example, is the imaginary activity the vermiculi acquire, in proportion to the time they remain in the semen, which would confute all I have

seen

seen and related in the first chapter. For, after being exposed to air, their motion becomes much more languid: before this, it is quicker: and its greatest rapidity is, when the semen is in the body of the animal, which, as I have observed, was remarked by Lewenhoek. Finally, M. De Buffon says, he observed those phænomena three or four days after the semen proceeded from the animal, and even upon the eighth day, in that of the rabbit. But this must be impossible: because, it cannot happen that these vermiculi, like those of human semen, which live longest when exposed to air, could live during the time mentioned by M. De Buffon; and, in this state, they could not live above seven or eight hours. When secured against the influence of air, in glass tubes hermetically sealed, they do not live three days, as we shall afterwards see. It is therefore decided, that the phænomena observed by the author in his experiments, could be exhibited only by the animalcula developing in the seminal fluids when on the verge of corruption, or when they do corrupt, as happens to other liquids which will corrupt, or are already corrupted.

I cannot suppress my surprize, that M. De Buffon never doubted that the animated beings he saw in semen, were really spermatic vermiculi, or only animalcula originating there; I mean

mean to ſay, the animalculæ of infuſions. He well knew, that this laſt ſpecies of animals originates no leſs in animal, than in vegetable ſubſtances, when they begin to corrupt. For he ſays, that in two infuſions, one made with the teſticles of a ram in water, and the other with thoſe of a dog, he, in ſome days, found living animals ſimilar to thoſe he had ſeen in the ſemen of animals; that is, globular or ovular vermiculi, without tails, moving with great activity, and often changing their ſhape. If the animalcula of the two infuſions were perfectly ſimilar to thoſe he had obſerved in ſeminal fluids, how did he not ſuſpect, that, inſtead of being ſpermatic vermiculi, they might have been infuſion animalculæ? He had an additional cauſe to think ſo; for he muſt have remarked, that thoſe changes of figure, diviſions, diminutions of ſize, were not to be ſeen in recent ſemen, but in that kept for ſome time, and about to corrupt. Of this, he muſt have had certain indications, and an indubitable proof, from the fœtid and cadaverous odour the ſemen then exhales; which is alſo a convincing proof, that animalcula are produced in it, upon account of its putreſcence; and conſequently, that we cannot confound them with the ſpermatic vermiculi. Another remark might have occurred, and might have ſaved him from miſtake, had he choſen to

make it, was, not to examine the ſurface of the ſeminal fluid only, but alſo the bottom. There, he would have found the vermiculi entire, although dead; which would have demonſtrated, that the animals he then ſaw in motion could not be ſeminal vermiculi.

But all I have hitherto ſaid againſt the theory of M. De Buffon, receives greater weight from the compariſon I have drawn between the ſpermatic vermiculi, and the animalcula found in putrid ſemen. In another work, I have ſhewn, that a conſiderable part of the animalcula of infuſions, appear, before the microſcope, an aggregate of minute veſicles, ſmaller or larger, and in greater or leſſer number; that the veſicles are inveſted by a common pellicle, which forms the exterior of the animal; that the pellicle and its veſicles are loſt and deſtroyed when the animalcula die; and if, while alive, they are wet with urine or vinegar, the body is deſtroyed, and reduced to nothing [a]. All theſe ſingularities are amply exhibited and verified, in the putredinous animalcula of ſemen; but, with all care and attention, I have never been able to ſee any thing like ſeminal vermiculi. The texture of the body and tail is not vaſcular: it is uniformly homogeneous; equally ſolid and compact. For this reaſon, perhaps, the vermiculi, when dead, fall to the bottom of the ſeminal fluid; and the infuſion

[a] Saggio de obſervazioni microſcopiche.

fusion animalcula commonly swim. Likewise, the vermiculi continue entire, long after death: urine, vinegar, even ebullition, cannot dissolve or decompose their contexture. We must therefore conclude from all this, and from all that has been already said, that the animalcula of infusions, and consequently those of putrid semen, are of a nature and constitution essentially different from those of spermatic vermiculi. We may easily determine that this must be real, since a fluid, which affords a salutary dwelling to the one, is fatal to the other. The putredinous seminal animalcula develope and live in corrupted semen, but they die in that which is recent and entire. On the other hand, the vermiculi live with safety in recent semen, but they die when put in that which is corrupted. The animalcula become more lively and more active when water is put in the semen; the vermiculi, at least several species, then become motionless and die. Of all these facts I have often convinced myself. Whence I conclude, infusion animalcula, and spermatic vermiculi, to be two species of animals, which we cannot confound without offending nature.

CHAP. V.

As I have taken the liberty respectfully to remark the errors of M. De Buffon concerning this subject, I must still request permission to show the deductions which may be made from his noted theory of organic molecules. But, to perform this with the greater success, it will be necessary to bring under our view, some of the chief points of that theory.

The illustrious French naturalist supposes, that every animal and vegetable substance includes a number of organic molecules, that is, active and incorruptible particles. He thinks they serve for the nutriment, encrease, and multiplication of all beings, whether they enter the body of animals by means of food, or that of plants with the sap they imbibe from the earth: that they intimately penetrate every part, there unite, and are identified, if we may say so, and afford nourishment to the plant or animal. If the plant or animal is young, it appropriates all the organic molecules, incorporating them with itself. The molecules, expanding, extend their fibres, and by this means effect increment. But if the being is already an adult, if it is no longer susceptible of expansion, then all the molecules, being

being unemployed in nutrition, thoſe which are ſuperfluous are depoſited in the organs of generation, and ſerve for the propagation of the ſpecies, which takes place when the organic molecules of the male are mixed with thoſe of the female; ſo that the moſt analogous tend to approach each other, in virtue of certain relations, and form ſingular wholes, reſembling in miniature the different parts of the two individuals in which they are modelled. From the wholes together, there reſults a general whole, which is the embryo.

If the organic molecules, afforded by the male, are more numerous or more active than thoſe of the female, the embryo will be a male; if the molecules of the female are more numerous, it will be a female. The greater number afforded by the male or the female, will occaſion the greater reſemblance of the embryo to the individual from which it has received them.

Large animals are leſs fruitful than ſmall: the reaſon is evident. The former extract fewer organic molecules in nutriment than the latter. A bull draws leſs nutriment from hay and ſtraw, and conſequently fewer organic molecules, than a bee does, in proportion, from the fineſt parts of flowers. Animals covered with ſcales are more fertile than thoſe covered with hair; probably becauſe the former per-

ſpire leſs than the latter, and becauſe they accumulate a greater number of organic molecules.

If, inſtead of collecting in the organs of generation, the molecules are carried to other parts of the animal, there they form minute living animals, as the *teniæ* and *aſcarides;* worms ſometimes inhabiting the inteſtines, the liver, and the ſinuoſities of the brain.

It is thus that M. De Buffon, in his theory, explains thoſe phænomona, and ſome other, which, for the ſake of brevity, I ſhall omit. As he wiſhes this theory, the offspring of his genius, to be adopted by nature, he recurs to the ſeminal fluids of animals, and the infuſions of plants, becauſe, in both, according to his opinion, are organic molecules, clearly exhibited under the form of globular, ovular, or other ſhaped corpuſcula, endowed with motion, ſubjected to various changes of figure, may be decompoſed into ſeveral ſmall bodies, and, having ſo much the greater activity, are further decompoſed, until their minuteneſs renders them inviſible.

This laſt trait of M. De Buffon's theory, proves that it reſts entirely upon the facts related by its author, that is, upon a falſe hypotheſis. For with reſpect to infuſions, we have already ſeen, that there is in them nothing to indicate organic molecules, ſince the moving corpuſcula

corpuſcula there, are actually animals, ſome of which are viviparous, and others oviparous; and becauſe thoſe multiplying by diviſion, do not produce that progreſſion of ſucceſſive diminution in ſize, which M. De Buffon imagined he ſaw: but the ſmalleſt encreaſe like other animals.

Having found the living putredinous beings of ſemen, to be preciſely of the ſame kind as thoſe of infuſions, by a direct and concluſive conſequence it follows, that they could never be confounded with organic molecules, and we may ſay the ſame of ſeminal vermiculi, whoſe animality I have ſufficiently proved by the facts already related in this treatiſe, and by thoſe which are engroſſed in the ſubſequent chapters.

The theory of M. De Buffon is thus completely deſtroyed. Such is too often the fate of thoſe hypotheſes, favoured by the inventive imagination of ſome philoſophers, and which they afterwards ſearch for in nature. This ingenious naturaliſt, diſſatisfied with the theories of generation already framed, and hurried on by his taſte for novelty, imagines in the body an animated matter, original, incorruptible, and always active, which he ſpeciouſly denominates *organic molecules*. Making them act according to certain terms, and with a certain effect, he thinks he can explain the great work of generation, and the moſt inſcrutable phæ-

nomena it prefents. In this attempt, he ufes all the powerful and perfuafive eloquence which characterifes him as the orator of the age. Prejudiced in favour of his theory, it was not difficult for him to find it in nature. His views were lefs directed to fee what actually exifted, than to what he wifhed to find. Like his celebrated countryman, the Reformer of Botany, who fancied that metals and ftones vegetated, and thought he had evidence of this imaginary vegetation; that he faw feeds and plants, where there were none. If this learned academician, who has ever poffeffed my full efteem, would take the trouble to repeat his experiments, upon the femen of man and animals, with the beft microfcopes, and, forgetting the organic molecules, fo dear to him, by impofing a rule upon himfelf, to receive, as truths, but the images tranfmitted by the fenfes, without adding the corrections of his imagination, as is the duty of a veracious naturalift; I may affure him, that he will find all I have fo largely defcribed in my works. I earneftly entreat him, not to reject this without a trial, which muft certainly refult to the advantage of truth.

I mean now to examine M. De Buffon's objections, to prove that the living beings feen in fpermatic fluids are not real animals. Some of thofe objections I have already anfwered in the beginning of chapter 3. And it appears, from what

what I have hitherto said, that some of them are false; such as, the formation of the animals under the observer's eye, the loss of the tails, and the diminution of their size. Two yet remain, of which I have not spoken; their division into parts, and their frequent change of shape. These phænomena are real, although, as we have seen, they are never observed in seminal vermiculi, but only in the animalcula of putrid semen. M. De Buffon esteems such phænomena incompatible with the state of animals.

With regard to the objection concerning the division of the animalcula, I judge it needless to stop here for a discussion; and I pass to the second, which relates to the metamorphoses of the putredinous animalcula. It is true, that the body of the animals familiar to us, is so constructed, that the shape never varies, and cannot be materially altered without destruction. But it is no less true, that there are others, and even many, experiencing the contrary; as for example, among insects, several species of worms. To be convinced of this, it is only necessary to open the works of naturalists, and to glance the works of nature. Several worms of this kind have been elegantly described by Rhedi and Vallisnieri. In short, a single species may serve for many: this may be the *cucurbit worm*. Is it not certain, according

ing to the obſervations of thoſe two, and eſpecially of the Tuſcan philoſopher, that they aſſume various ſhapes, and, in the words of this learned naturaliſt, "ſometimes they contract "and ſwell like purſes, ſometimes they extend "and curve in a ſemicircle?" Does not Reaumur ſay the ſame of certain worms changing to flies, whoſe head, that part conſtantly the ſame in moſt animals, in this inſect, ſo wonderfully changes its appearance, that it is ſometimes extended, ſometimes depreſſed, ſometimes contracted, ſometimes obtuſe in one part, and acute in another? Do not we daily ſee the ſame changes in earth worms, ſnails, and particularly in leeches; extending the body till it becomes long and ſlender, then contracting it till it becomes ſhort and corpulent; grows thick at one end, and ſmall at the other? What ſhall we ſay of the wheel animal, that aquatic creature, which, from its wonderful and incredible metamorphoſes, we may term the Proteus of the inſect tribe? If wheel animals, leeches, ſhell and naked ſnails, ſo many ſpecies of vermes, are not degraded from the quality of an animal, notwithſtanding their metamorphoſes, the ſame ſhould be the caſe with the animalcula of putrid ſemen.

I have ſtill to appretiate two other objections. One is deduced from the ſingular motion of ſeminal vermiculi; the other from the different

different effects of heat and cold upon them. Let us attend to the firſt. An animal, whatever it is, according to the reaſoning of M. De Buffon, is ſubject to change its inclinations; ſometimes its motion is quick, ſometimes ſlow; then it ſtops and reſts. But nothing of this is ſeen in ſpermatic vermiculi: they are in continual motion, they never ſtop, they never reſt; and when once they ſtop, it is for ever. Hence, they cannot be animals.

This objection is like the former, both being founded upon the analogy of the large animals beſt known to us; but as they preſerve their ſhape, they likewiſe have eſſentially the viciſſitudes of reſt, of motion, and of an accelerated or retarded motion, &c. But, to be certain that ſuch viciſſitudes are a characteriſtic quality of animals, it is improper to attend to large animals only; but it is neceſſary to examine other genera, and other ſpecies, and to dwell particularly on the ſmalleſt, eſpecially thoſe inhabiting fluids, which have more analogy to ſpermatic vermiculi. It is certain, that among them, M. De Buffon would have found ſeveral animals, which, far from having the alternatives of motion and reſt, are naturally in motion, ſo that their life ſeems to conſiſt of a perpetual motion. To be aſſured of this, it is ſufficient, during the Spring, to obſerve the water of marſhes, ponds, and ditches, where

all

all insects are. There we shall see some in motion, but by contortions: such, for example, are the worms mentioned by M. Trembley, serving the polypi for food; the body is in a constant oscillation *. But, without the trouble of seeking them in the country, M. De Buffon may, at his leisure, observe them at home. The eels generally found in vinegar will evince it. If a small portion of this liquid is put in a thin glass vessel, and opposed to the light when the sun is bright: examining with a magnifier the higher parts, where the eels are more distinctly seen: their contortions appear incessant; they dart from one side to another; and this continues, without intermission, from morning till night. Thus, they persevere for several months; that is, till the end of their lives. It would seem, that this perpetual motion cannot be a sufficient reason to prove that spermatic vermiculi are not animals.

But, farther, such a motion is not natural to the vermiculi; it is forced and violent. When they have quitted their natural abode, to enter our atmosphere, they experience the lively impression of the air, which pains them, and obliges them to perpetual flight. Doubtless, the air is noxious to them, and occasions their continual motion, as is proved by the facts. To ascertain the point, I waited until the human semen

* Memoires sur les Polypes. Mem. second.

ſemen cooled; I then took ſome drops, ſpreading them very thin upon the glaſs of a watch. It conſtantly happened, that the vermiculi of the ſpread drops, although of conſiderable thickneſs, periſhed ſooner than thoſe of the complete portion; becauſe, as I think, the former were more expoſed than the latter to the air. At the ſame time, I put two equal portions of the ſame ſemen, one into a cloſe, the other into an open veſſel: the vermiculi in the latter always died ſooner than thoſe in the former. The privation I made them undergo, proved well how injurious this element was to them. They lived longer in vacuo, than in the open air; ſo that when in vacuo they were ſtill alive, all were dead in the open air. The difference of time at which they died was an hour and a half, two, even three hours, and ſometimes longer, according to the ſeaſons when the experiments were made. Theſe facts demonſtrate, that the air is noxious to the vermiculi, and the following prove that it is the cauſe of their being in continual agitation. With the blowpipe I formed capillary tubes, one end of which I immerſed in recent ſemen: it aſcended the cavity, filling the tube to a certain height. Breaking the tubes near the part to which it aſcended, I preſented this extremity to the blowpipe, and immediately ſealed it hermetically. I did the ſame to the other end, by which

means

means the ſeminal fluid was deprived of all communication with the external air. I drew out the tubes ſo, that the thinneſs of the glaſs permitted me to ſee the vermiculi within. The peculiarities preſented by the vermiculi in the tubes, were very different from thoſe of the reſt. All, or at leaſt moſt part of them, had a ſingular mode of moving. Some had that ſort of activity obſerved in thoſe which experience the influence of the open air. Others had a continued irregular motion; they changed from quickneſs to inactivity, and reciprocally. Others ſtopped entirely, and, after reſting ſome minutes, reſumed their former velocity: beſides, we did not ſee them inconſiderately running againſt the ſolid portions of the ſemen, as I remarked in the firſt chapter, but always avoiding them, turning aſide, or retreating. It is true, theſe peculiarities always ſucceeded better, and with more uniformity, when the tubes were kept warm. I have before ſaid, the longeſt period of life of the human ſpermatic vermiculi, was ſeven or eight hours, when expoſed to the open air; but this period is greatly prolonged when they are included in tubes. In Summer I have ſucceeded in preſerving them two days and more; and in Spring and Autumn they have lived almoſt three.

It is undoubtedly very ſurpriſing, that the vermiculi live longer in Spring and Autumn, than

than in Summer: the reverfe fhould take place; becaufe the heat of Summer fhould be more congenial to them, as it approaches nearer to the natural heat of man in life. At firft it gave me confiderable furprife, which increafed upon reflecting, that in the open air the vermiculi live much longer when the weather is warm. This circumftance induced me to repeat my experiments; but I conftantly found, that during Summer, they never lived as long in the tubes, as during Spring and Autumn. In Summer, I may even fay, that they die fooner upon the warmeft days. With a little reflection, it is not difficult to comprehend the caufe of the difference. We have feen, that the femen of man, and of animals, when removed from its native fituation, very foon becomes putrid; and that this happens fooner, as the heat is greater to which it is expofed. It is to this caufe, therefore, that I afcribe the more immediate death of the vermiculi, in capillary tubes, during Summer. Having filled fimilar tubes with recent femen, and fealed them hermetically, I expofed fome to the heat of the atmofphere at about 63°, and others to the heat of the human body, keeping them under the arm-pit in a large glafs tube. My own heat, when in a ftate of health, is about 97°. The vermiculi expofed to the heat of the atmofphere, lived two days and a half; fome even three: but thofe

those experiencing the heat of my body, supported it only from eleven to thirteen hours. This more immediate death cannot be ascribed to the greater degree of heat they suffered, since it is inferior to that they usually experience, or in which they naturally live. Nor can it be occasioned by the air; for the tubes being hermetically sealed, the air can have no influence. We cannot ascribe it to any other noxious principle arising from the nature of the tubes, since they were absolutely similar to those exposed to the atmospheric temperature, in which the vermiculi live much longer. It is therefore necessary to recur to some alteration, or some noxious quality contracted by the semen, which makes the heat accelerate their death; and I acknowledge, that I cannot find this alteration, or this bad quality, but in the principle of putrefaction of the semen, which is manifested by the foetor experienced upon breaking the tubes. It is doubtless this odour which must be fatal to the vermiculi, as I have demonstrated in chapter 6. The putrefying principle does not take place in semen exposed to the open air seven or eight hours in Summer, as I have convinced myself; but, upon the other hand, the heat of Summer, more nearly than that of any other season, approaches the heat which the vermiculi experience in us. Thus, we clearly see, why they live

live longer in the open air during Summer, than at any other time; of consequence, we may comprehend why their life is abridged in proportion as the cold encreases.

But it is time to come to the objection of heat and cold, which our author thus proposes. Upon exposing the seminal fluid to the cold air, the vermiculi did not seem to suffer from it; and they continued to move with their usual quickness, as long as those that were not exposed, although the seminal fluid had acquired that degree of cold, in water upon the point of freezing, as one may be convinced by touching it. Upon the contrary, if the same vermiculi experience heat, their motion ceases, although the heat is moderate. In consequence of these facts, if the vermiculi are real animals, (it is M. De Buffon's reasoning I relate), they would present an appearance and constitution very different from the appearance and constitution of other animals; as too great a degree of cold relaxes and destroys their motion, and a mild and moderate heat preserves their activity.

It is to be regretted, that our author, instead of employing taction to judge of cold and heat, did not use a thermometer; for all philosophers know, that the touch is a very equivocal proof. He ought to have discovered precisely, at what degree of heat the motion

of the vermiculi ceafes, and what is the degree of cold by which it is not relaxed. I have therefore efteemed it moft effential to fupply this defect, in order to obtain arguments more conclufive and decifive, and to know, if, with refpect to heat and cold, the vermiculi are of a nature and conftitution differing much, as M. De Buffon imagines, with the nature of other animals.

Although the obfervations of chapter 1. do not feem to beftow upon them that vigour of conftitution fufficient to enable them to refift the cold, becaufe it appears that their motion decreafes with the heat of the atmofphere, fo that, when the thermometer ftands at 36°, they continue to move only an hour and a half; I refumed my experiments, refolving to extend them further, and to fubject the femen to a degree of cold equalling freezing, carefully obferving what fhould happen to the vermiculi. The femen I ufed, was that of the horfe. Upon the 14. of January, I expofed it to cold: the vermiculi could not be more vivacious: but this diminifhed under the eye; and, in fixteen minutes, all were motionlefs, although the femen was not frozen.

The cold becoming more intenfe, upon the 18. I repeated the experiment. The vermiculi became motionlefs in eleven minutes; the thermometer

mometer ſtanding 7° under freezing. The ſemen was ſtill fluid.

I often repeated thoſe experiments during this Winter. They conſtantly proved, that the duration of the motion of the vermiculi, was proportioned to the temperature of the weather.

In Summer, I proſecuted my experiments upon the ſemen of the horſe. Then a thought ſtarted, of ſubjecting the vermiculi to the cold of freezing, by putting the glaſs in which they were, among ſnow. The ſame effect was produced by ſnow, as by the Winter's cold; that is, in fourteen minutes, it made them motionleſs; although, when expoſed to the heat of the atmoſphere, they continued to move ſeven hours and a half. But an accident that happened in this experiment, executed during Summer, afforded new intelligence, and diveſted me of a prejudice. Obſerving that the vermiculi had become motionleſs, I took the glaſs from the ſnow, and left it expoſed to the air, when the heat was 81°. An hour after, by chance obſerving this ſemen with the microſcope, I was aſtoniſhed to find all the vermiculi reanimated, and in ſuch a manner, as if they had juſt come from the ſeminal veſſels. I then ſaw, that the cold had not killed them, but had reduced them to a ſtate of complete inaction. I replaced them in the ſnow, and in three quarters of an hour took them away.

 These

These are the phænomena I observed. In a few minutes, their vivacity relaxed, and the diminution encreased, until they lost the motion of progression, and retained only that of oscillation, which likewise ended in a few minutes more. Exactly the reverse was observed, when they passed from the cold of the snow to the heat of the atmosphere. The first motion that appeared, was that of oscillation: the body and the tail begun to vibrate languidly from right to left: then the motion was communicated to the whole vermicule; and, in a short time, the progressive motion begun. At first, it is scarcely perceptible; it soon encreases, and becomes very considerable. I may also add, that as cold does not destroy the motion of all the vermiculi at the same moment, but of some later than others; so does not heat affect them all with equal power.

I subjected the semen of man, and of the bull, to the same experiment, and had the same results as from the semen of the horse, excepting only that a degree of cold, less than that of freezing, destroyed all motion in the vermiculi of the bull.

Upon the approach of the following Winter, I resumed the same experiments, and I succeeded in reanimating the torpid vermiculi, by breathing upon the semen, by applying the finger to the talc upon which the drops were put, or by

by placing it near the fire. When removed from this heat, they fell into the ſame lethargy as when in Summer: they paſſed from the atmoſpherical temperature, to the cold of the ſnow. During this rigorous ſeaſon, I expoſed our vermiculi to a more ſevere trial. I ſubjected them to cold, more than 9° under freezing. As I expected, this immediately made them motionleſs. In five minutes, not a ſingle vermicule moved. When they had been expoſed five minutes longer, I tranſported them into warm air, leaving them there for ſome time. Although this intenſe cold continued ten minutes, the ſemen was not frozen; but it had fatally injured a complete third of its inhabitants, which exhibited no ſign of life, when removed to, and kept long, in a warm ſituation; on the contrary, they had all the appearance of death. The other vermiculi recovered, indeed; but their motion was languid, in compariſon to what it was before. This experiment was made 27 December, and I repeated it upon the evening of January 5, at a degree of cold 10° under freezing. I perceived, that in about a quarter of an hour, the ſemen begun to congeal about the edges of the glaſs. I then put it into a ſtove, but this had no effect upon the vermiculi. Not one recovered, and thoſe enveloped by the ice periſhed, as well as thoſe in the fluid part. The ſame happened to the

vermiculi of other two glaſſes, upon which I made the ſame experiments this evening; although I took care to regulate the different degrees of heat, leſt too ſudden a tranſition from cold to heat might be injurious.

Such were the experiments made by cold: any one may draw the concluſions. Very far from excluding the vermiculi from the rank of animals, it ſurpriſingly confirms them in it: for, what can better prove animality, than languor and loſs of motion, when affected by cold? What effect can more ſatisfactorily evince it, than to ſee this languor more immediate, as the cold is more intenſe; and to ſee the vermiculi revived, when brought to heat; and to witneſs their actual death, when the cold is of a greater degree? Such is the ſtate of the greater part of ſmall animals, when deprived of action, and rendered torpid by cold: with heat they recover life and motion, and yield under cold ſtill more intenſe.

But how can theſe facts, multiplied, repeated, uniform, conſequently certain and incontrovertible, ſubſiſt with the aſſertion of M. De Buffon, who thinks that cold does not impede the motions of the ſpermatic vermiculi? Inſtead of negativing the aſſertion of this illuſtrious Frenchman, I think there is a method of conciliating our obſervations. We have already noticed the error which occaſioned his confounding the ſe-

minal

minal vermiculi with animalcula, aſcribing to the vermiculi thoſe properties pertaining to the animalcula only.

It is very likely that the effect of cold, which, he ſays, he has obſerved upon vermiculi, is alſo a conſequence of the ſame miſtake; and this is the more probable, as it is ſeen in the putredinous ſeminal animalcula. Not only do the animalcula of infuſions, at leaſt ſeveral ſpecies of them, withſtand cold of a great degree; but thoſe found in putrid ſemen, are undoubtedly of that number. I am convinced of this from ſeveral experiments, which, to avoid the ennui of my reader, I ſhall not ſtop here to detail. But there is one circumſtance I ought not to paſs in ſilence. Although the animalcula can ſupport a greater degree of cold than the ſeminal vermiculi, yet their motions become more languid, and they periſh like the inſects which yield under the greateſt degree of cold.

When I found thoſe modes of conciliation, between the obſervations of M. De Buffon and myſelf, with regard to the phænomena from cold, I attempted to find the ſame reſpecting thoſe exhibited by the effect of heat. But this was impoſſible. My obſervations have been completely oppoſite to his. Thoſe of M. De Buffon are comprehended in a few words: "The motion of the vermiculi ceaſes, when "they are expoſed to a ſmall degree of heat."

I entreat the reader to examine mine, that he may be enabled to compare and form a judgement of them.

I placed upon the water of a veſſel two watch glaſſes, one of which contained a given portion of recent ſemen, full of vermiculi; the other, an equal quantity of the ſame ſemen, old, and ſwarming with putredinous animalcula. To know the ſucceſſive degrees of heat, I had put the ball of a thermometer into each glaſs. The water was gradually heated upon a ſlow fire. As the liquid in the thermometers aſcended, I took ſome drops of ſemen from the glaſſes, for examination with the microſcope. The putredinous animalcula lived at 99°; at 104, their motion begun to grow languid; and at 106 and 108, all periſhed. The ſeminal vermiculi are of a more hardy conſtitution. At 106°, they were very active; ſome begun to periſh at 120°, and at 131 there was not one alive; ſo that the difference which occaſions the deſtruction of the one and of the other, is about 22°. The vermiculi were thoſe of the human ſemen. I repeated the ſame experiments upon the ſemen of the horſe, the bull, and the dog. Thoſe of the horſe and the dog periſhed at 126°; thoſe of the bull at 133.

I varied the experiment. I filled ſome capillary tubes with ſemen, one part of which was full of ſeminal vermiculi, the other of the putredinous

putredinous ſeminal animalcula. I ſealed them hermetically, and put them at the bottom of a veſſel full of water, which was gradually warmed; I alſo immerſed the ball of a thermometer. When the water had attained 99°, I begun to examine the tubes, one after another. In this new experiment, the ſeminal vermiculi of man, and of the animals I have mentioned, died at only 122 and 124°; and the putredinous animalcula, at 106 and 108°.

Theſe facts demonſtrate, that, if we ſpeak of animalcula originating in putrid ſemen, they are of a conſtitution better calculated to reſiſt a degree of heat, which ſeveral other animals cannot ſupport, and are killed by heat, only when it arrives at that degree, or about it, which is fatal to putredinous animalcula. If we ſpeak of ſeminal vermiculi, we ſee, that inſtead of ceaſing to move, and periſhing at a ſmall degree of heat, according to M. De Buffon, they ſupport a degree which deſtroys ſeveral other animalcula. But this, far from being wonderful, is rather congenial with their nature, ſince they live in the bodies of warm-blooded animals; that is, in an atmoſphere in general much warmer than the air, and the other fluids where the reſt of animalcula are found.

CHAP. VI.

I FLATTER myself that the reader will not be displeased, if, in the rest of the observations, he does not find the same order and connexion I have before endeavoured to preserve. It is necessary to consider what follows as an appendix, which, I think, must indispensably be added, to prove, with greater certainty, the animation of the vermiculi, this being one of the chief objects I proposed; and, when once established, we shall not only see confuted the contradictory opinions that have been formed concerning the nature of seminal vermiculi, which have already been explained and discussed, but we may also anticipate every new hypothesis that may be suggested.

One day during Winter, I had a great quantity of semen then taken from a dead body [a]; and, wishing to preserve the vermiculi some hours alive, I put the watch-glass where they were in the sunshine without a window. The heat of the sun was 70°, which kept them alive a considerable time; but, observing the vermiculi an hour after, I was extremely surprized

[a] When the kind of semen used is not specified, that of man is always understood.

prized to find almoſt the whole motionleſs. I knew not whether this was the indication of real or apparent death; and, thinking to ſatisfy myſelf, by expoſing them to a greater degree of heat, I tranſported them near the fire. Experience had taught me, how inſtrumental the influence of heat is in reſtoring the vermiculi to life. But this was vain; and although I kept them at this degree of heat a length of time, they exhibited no ſign of life. It was otherwiſe with thoſe I had left in the ſhade, and then carried to the fire; for I had another portion of ſemen in a watch-glaſs, in the ſame apartment. The vermiculi had become motionleſs like the firſt; but they ſoon reſumed their original vivacity. What was ſurprizing in this phænomenon, ſeemed to me the effect of peculiarity; and I did not think of repeating the experiment during this Winter and the following Spring. But I had afterwards occaſion to obſerve, that the ſun, in a few hours, was conſtantly fatal to the vermiculi, although the intenſity of the heat did not equal that degree which is fatal to them, of which I have ſpoken in the preceding chapter. This I aſcertained, by means of the ſun in Autumn: but the phænomenon, which I at firſt thought accidental, has to me appeared conſtant and invariable. The influence of the ſun, at the ſame time, has no quality noxious to the pu-

tredinous

tredinous animalcula of the same semen, provided the intensity of the solar heat does not raise the thermometer to 106 or 108°; which also contributes to prove the difference between the vermiculi and animalcula.

The novelty of the results was sufficient to incite me to investigate the cause, experiment having shewn me, that a certain degree of solar heat quickly kills the spermatic vermiculi, although the same degree in an apartment does them no injury. I could not be persuaded that the simple heat of the sun occasioned this destruction. I imagined the cause entirely different. My first idea was, the agitation of the air. I thought that when I put the semen without the window, the vermiculi were more affected by the violent action of this element, and sooner yielded under it, than those within the apartment; at least, it was here less agitated. But this imagined cause was false: for I put two glasses, provided with the same semen, without the window, and equally exposed to the air; with this difference only, that one was in the sunshine, and the other in the shade. Those exposed to the sunshine, always died several hours sooner than those in the shade. Further, I put a division in the seminal fluid of the same glass, separating it into two parts; so that one was exposed to the sunshine, and the other was not. It always happened, that the

the vermiculi of this latter portion long survived those of the other.

Attentively contemplating the semen with the naked eye, I suspected another cause. I saw the semen not only greatly diminish, but become more dense, and change colour. I then thought that this grossness might be noxious to the vermiculi. To ascertain the fact, I employed an easy method, which was, to prevent the evaporation of the semen in the sunshine, because the density might be occasioned by the evaporation of the more volatile parts: and I attained my purpose, by hermetically sealing several capillary tubes full of semen, then exposing them in the sunshine, along with another portion of semen, in a watch glass. The heat of the sun raised the thermometer to 72°. The vermiculi in the watch glass did not live an hour; but those in the capillary tubes retained all their vigour at sunset, although the experiment was made in the morning; and an hour after mid-day, the heat of the sun equalled 104°. Upon the following days, I exposed to the sunshine other capillary tubes, prepared as above; the vermiculi were long alive. These repeated facts therefore prove, first, that the sudden death of vermiculi in the sunshine, is not properly the effect of the heat of the sun, as it would have killed those in the capillary tubes almost as soon as those in the open vessels; secondly,

condly, we cannot aſcribe their immediate death but to ſome vicious quality or alteration acquired by the ſemen, when expoſed to the air, and againſt which it is ſecured in a cloſe veſſel. But as it does not ſeem to ariſe from any other cauſe, than from the thickening of the ſemen, ſince putrefaction cannot begin in ſo ſhort a time, we are induced to ſuppoſe this thickening to be the ſole cauſe of their death, or at leaſt an eſſential reaſon.

Thoſe facts have been illuſtrated by the following. I placed in the ſun two glaſs tubes, filled with ſemen to a given height, and ſtopped with a ſtopper well fitted; with this difference only, that the ſtopper of one tube touched the ſemen, and that of the other was an inch above it. The tubes were placed erect, each containing an equal quantity of ſemen. In an hour and a half, the influence of the ſun had occaſioned no evaporation in the tube with the ſtopper touching the ſemen: it was indeed impoſſible, as there was no vacuity between them. But the ſemen in the other tube had evaporated. I ſaw the inſide of the glaſs covered with a thin pellicle, formed of a very tranſparent fluid, which could only be the more ſubtile parts of the ſemen volatilized by the heat. The quantity of the ſemen was diminiſhed, which could not be otherwiſe, as it was a little thicker: neither of which circumſtances was remarked

marked in the other tube. I examined the two femina with a magnifier. The vermiculi of that where no evaporation appeared, were full of vivacity; thofe of the other all were dead. Thus it is evident, that the folar heat does not kill the vermiculi, but that their death is occafioned by fome noxious quality imparted by heat to the femen; which either confifts in it becoming more grofs, or in fome other quality it thence derives, or which is produced upon this occafion. This circumftance alfo correfponds with the nature of the animals, which are injured and perifh, if the ambient fluid in which they live begins to alter or fpoil.

It yet remains to explain how two degrees of heat, different in effect, but equally intenfe, can have fuch oppofite effects: for the immediate action of the funfhine changes the feminal fluid in fuch a manner, that it kills the whole vermiculi; while the fame degree of heat, in a heated apartment, does them no injury. I have not made enough of obfervations to folve this problem.

We have feen, that the feminal vermiculi continue to move complete days in clofe veffels, and perifh in fome hours in the open air. I have fhewn, that this long continuance of life in clofe tubes, arifes from their being fheltered from the influence of the air. From analogy one would think, that this might occur at all

seasons. But arguments drawn from induction should not be used by philosophers, since they are often supported by deceitful facts: the present case may afford a new instance. During Winter, the vermiculi, in tubes hermetically sealed, become motionless in the same time as those in watch glasses exposed to the open air: that they should become motionless, the cold of freezing is not required. In an hour and a half, I found them motionless in the tubes, the same as in the open air, when the thermometer was at 45°. That the vermiculi may live in small tubes, a certain degree of heat is necessary, which my experiments indicate to be 52 or 54°.

When the vermiculi become motionless from cold in open vessels, it is not always a sign of death: sometimes it only indicates a simple lethargy. When included in tubes, I have, by means of heat, from perfect rest restored them to their natural motion. I have even produced this rest and motion successively, by transporting the tubes from heat to cold, and *vice versa*. We must remark, that the repetition of this operation enfeebles the vermiculi so much, that in a certain time they cannot recover motion, and perish for ever.

I wished to learn how long the vermiculi could remain lethargic without destruction, so that, passing them to a warm situation, they might still

ſtill reſume their power and motion. I have found myſelf unable to determine the limits of this; but it appears in a great meaſure to depend upon the degree of cold to which they have been ſubjected. If the cold ſurpaſſes freezing, and we delay ſome hours to remove them to a warm ſituation, then they revive no more, or but a few are re-animated, and theſe are generally very weak and ill. If the cold is leſs intenſe, and the thermometer falls only to 39 or 41°, they may remain lethargic fourteen hours, and even longer. I do not intend here to demonſtrate to the reader, that the accidents happening to vermiculi in capillary tubes, completely quadrate with thoſe experienced by animals expoſed to cold, and that cold is fatal to them as to many inſects: this may be underſtood without explanation. If we ſhould now unite theſe traits, with the reſt diſperſed throughout this work; with the death of the vermiculi cauſed by poiſonous exhalations, by the odour of camphire, the oil of turpentine, the fumes of ſulphur and tobacco, by the effluvia of moſt ardent liquors, and the electric ſpark, as I have proved; we ſhall have an aſſemblage of proofs, ſo convincing, ſo deciſive of the real and abſolute animality of the ſeminal vermiculi, that I know not what other evidence could be re-

quired, to prove that atoms ſo minute are of ſuch a nature as the vermiculi ſeem to be.

I wiſh to excuſe the inconſiſtency between what I have hitherto ſaid of ſeminal vermiculi, and the little I ſaid of them in my firſt treatiſe upon infuſions. There, I ſpoke careleſsly of them; I had not then ſtudied them; I had conſulted what others had written upon them, and adopted the theory which ſeemed to be beſt ſupported by facts. I did not heſitate to adopt the opinion of M. De Buffon, and with him ſuppoſed the vermiculi not to be real animals; as this opinion appeared to be ſupported by obſervations more numerous, better detailed, more connected and convincing, than thoſe of Lewenhoek. Then, I thought thus, and ſhould always have thought the ſame, had not the obſervations I have related convinced me of the contrary; and I flatter myſelf, that I ſhall not be reproached if my former opinion was different from that I now have.

This chapter ſhall be finiſhed with reflections upon ſome queſtions, as curious as nice, reſpecting our vermiculi. To me they were communicated in a letter from M. Bonnet; and the reader cannot better judge of them, than when he has them before his eyes. After informing me of the ſingular opinion of Linnæus, that the vermiculi are inert corpuſcula floating in the ſemen, he adds, 'I return to

'the

'the ſeminal vermiculi, and I cannot doubt 'their exiſtence. They are, of all the animalcula of liquids, thoſe which have moſt excited my curioſity: the element in which they 'live, the place of their abode, their figure, 'motion, their ſecret properties, all, in a word, 'ſhould intereſt us in ſo ſingular a kind of minute animated beings. How are they found 'there, how are they propagated, how are 'they developed, how are they fed, and what 'is their motion? What becomes of them 'when the liquid they inhabit is returned by 'the veſſels, and mixed with the blood? Why 'do they appear only at the age of puberty; 'where did they exiſt before this period? Do 'they ſerve no purpoſe, but to people that 'fluid where they are ſo largely ſcattered? 'How far are we from being able to anſwer 'any of theſe queſtions! And how probable 'it is, that future ages will be as ignorant of 'the whole, as our own! if, as I have ſaid, 'part 12. and 13. of the *Palingeneſie*, our 'world has been chiefly made for underſtandings, which enquire into the hiſtory of ſpermatic vermiculi, and that of the moſt myſterious productions of the globe. You may 'ſee, in articles 131, 132, 134, 135, of my '*corps organiſés*, that, in my youth, I attempted to conſider our animalcula. Obſerve what 'is ſaid upon this occaſion in article 135, con-

'cerning the animalcula of infusions: "Re-
"specting the appearance of animalcula in sub-
"stances which have been boiled, or subjected
"to a degree of heat, at which we cannot con-
"ceive that any animal may live: the difficul-
"ty ought not to surprise us too much, as it
"is founded only upon our ignorance of the
"heat which certain animals may support.
"Besides, it is not certain, that those animal-
"cula were in the infused substances. Per-
"haps they might inhabit the air confined in
"the vessel, and pass from it to the infused
"matter. Perhaps there is a perpetual circu-
"lation of those aerial animalcula in organised
"bodies, and in bodies organised in the air."

'I know no animalcula more fit than semi-
'nal vermiculi, to demonstrate how well it
'pleases the Supreme Wisdom to multiply sen-
'tient beings, and to leave no portion of na-
'ture void. Could we have suspected, that
'this precious liquid, the reproductive principle
'of large animals, was at the same time destin-
'ed for the aliment and pleasure of an innu-
'merable multitude of most minute animated
'beings? It is thus, that this Adorable Wis-
'dom has presided over the formation of the
'universe, and has known to make the same
'production serve for such different purposes.
"The Author of Nature," have I said, *Contemplation, partie* 5. *chapit.* 17. "has left no-
"thing

" thing uſeleſs. The pollen conſumed in the
" generation of plants is very little, compared
" with the whole quantity each flower affords.
" Wiſdom has, therefore, created the induſ-
" trious bee, which uſes the ſuperfluous part
" of this duſt, with an art which the moſt
" ſkilful geometers know not ſufficiently to ad-
" mire. The pollen of the ſtamina apparent-
" ly ſupplies the neceſſities of many other in-
" ſects." And thoſe inſects are in ſome re-
' ſpect to the pollen, what the ſeminal vermi-
' culi are to the ſeminal fluid.

' The origin of certain worms in the human
' body, and in the body of animals, is a pro-
' blem as yet unſolved by naturaliſts. Such,
' in particular, is the origin of the *tænia*. I
' ſpoke at length of this ſingular worm in my
' diſſertation. The origin of ſpermatic vermi-
' culi is a problem ſtill more profound. How-
' ever, I ſhould much incline to preſume, that,
' as thoſe I have mentioned in my diſſertation,
' they originate from without. The change of
' temperature, abode, and nutriment, may pro-
' duce, firſt in individuals, and then in ſpecies,
' very material alterations, to our eyes diſguiſing
' the primitive appearance. A worm deſtined to
' live in the waters, and tranſported to our in-
' teſtines, might not periſh, and yet be very
' much diſguiſed, eſpecially if introduced when
' young, or under the form of an egg, or of

'semen: and, if the worm was to propagate, 'the fubfequent generations would be ftill 'more difguifed. Let us fuppofe, therefore, 'that the femina of certain infufion animalcu-'la may be introduced into the feminal refer-'voirs, by the circulatory ducts: they might 'be developed and live there. There is no 'doubt, that this new abode, a temperature 'and aliment fo different, may greatly affect 'the original form of the animalcula; and at 'length occafion changes, which may more 'and more remove them from their firft ap-'pearance. All mankind had the fame origin. 'What varieties, and ftriking varieties, are 'there in the human fpecies! Let us com-'pare the inhabitants of the Frigid zone, with 'thofe of the Temperate region; and thofe of 'this, with the inhabitants of the Torrid zone; 'and we may fee the different fpecies of men. 'The femina of certain infufion animalcula 'are probably fo minute, that they may eafily 'arrive at the refervoirs of the feminal fluid. 'Apparently, they are excluded only in thofe 'feminal liquids that have acquired a certain 'perfection; which happens only at the age 'of puberty. It would be a moft curious ex-'periment, to try whether the animalcula of 'infufions would live in fome feminal fluids: 'and, in the fame manner, to try whether the 'feminal vermiculi would live in infufions.

'It

'It would, above all, be neceſſary to regulate 'the temperature of the place, and of the fluid. 'Who can ſay, that this experiment, which is 'certainly very new, will not ſucceed. I com- 'municate to you all the ideas that ſuggeſt 'themſelves to me. My maxim, in natural 'hiſtory, is to deſpair of nothing; and to in- 'terrogate nature in every way, even the moſt 'uncommon. Why ſhould we ſay a thing is 'impoſſible, becauſe we have not ſeen it ſuc- 'ceed? I found my maxim upon our pro- 'found ignorance of the ſecrets of nature, and 'upon the deviations which, in many caſes, 'ſhe ſeems to make from her ordinary courſe. 'Every where I ſee an univerſal latitude, the 'limits of which I am ignorant. They can be 'diſcovered by experiment alone. And how 'much may experiments of every kind be 'combined, multiplied, repeated, and per- 'fected!'

The difficulty of the queſtions propoſed in this valuable extract of a letter, is too well defined by its illuſtrious author, not to be ſeen by one who has the ſmalleſt portion of philoſophy. It will always afford me a good excuſe, if I only attempt to anſwer the doubts by diſtant conjectures. The queſtions may be reduced to three. 1. What is the origin of the ſeminal vermiculi? 2. How do they propagate? 3. What purpoſe do they ſerve?

I return to the firſt. Although M. Bonnet makes no aſſertion, we nevertheleſs perceive his inclination to think the vermiculi have an external origin. Such has been the opinion of many authors in eſteem; and ſuch is the opinion of thoſe who think that the worms in the body of man and animals originate from without. Sir Charles Linnæus believes the abode of the *tenia* to be in the waters; there he has found them very ſmall, and even in ſome fiſhes, particularly in tench, which ſeems to favour this opinion[a]. But we ſhould be certain of the identity of the ſpecies of tenia found in the waters, with that found in the human body; and of this we have not yet ſufficient proof. We cannot deny, at the ſame time, that very certain obſervations demonſtrate that ſome worms of the human body, at leaſt of the bodies of particular animals, are actually produced by inſects of the great world. Such are thoſe inhabiting the rectum of the horſe, the frontal ſinus of ſheep, goats, and the larynx of ſtags; by the diſcoveries of the celebrated naturaliſts, Valliſnieri and Reaumur.

With reſpect to ſeminal vermiculi, my obſervations will not allow me to aſcribe them to an external origin; for, was it ſo, certainly I muſt once have perceived it. More than fourteen years have elapſed ſince I have been occupied with

[a] See the Italian tranſlation of La Contemplation, part. 10. chap. 26. Note, at the words *molte centenaia de piedi*.

with infufions, fince I have ftudied the waters of marfhes, ponds and ditches. I can however fay, with abfolute fincerity, that among the innumerable multitude of minute animals, there are none refembling the feminal vermiculi of man and other animals [a]. I do not deny, that by fuppofing they pafs from the water to animated bodies, they may poffibly undergo fome change or alteration, operated perhaps by the caufes the Genevefe philofpher details, which he has rendered probable by demonftrating, that animals changing their climate and aliment, fuffer an alteration: "Ranæ in Ebufum infulam "delatæ colores mutant; aves in regione Sep-"tentrionali albefcunt, in Meridionali nigref-"cunt: fic vulpes, urfi, lepores mutato loco, "colores et quandocunque mores mutant." I alfo acknowledge, that the form of the body of vermiculi may change in its proportions; that the

[a] This had been already remarked by Lewenhoek, as appears in his 301. letter: "Licet varias et indole diver-"fiffimas aquas contemplatus fim, nec iftius modi animal-"cula (id eft fpermatica) nec quidquam quod animalcula "ifta fimilitudine aliqua vel figura referret in ullis unquam "aquis obfervaverim." Other obfervers agree as much as I could defire, with Lewenhoek and myfelf. Among 146 genera which Muller has claffed, he has only feen one refembling the fpermatic vermiculi of the fheep. It is that he calls *Cercaria*. But this fpecies, which is the only one that has been obferved, and very feldom found, (in infufione animali raro), would be far from being able to give exiftence to all the different fpecies of thofe vermiculi.

the parts may become larger or ſmaller, according as the new element agrees with their nature: but I cannot think they will loſe their priſtine figure, to aſſume one very different; or that the firſt change will be ſuch as to prevent them being recogniſed; for then the internal ſtructure muſt be changed alſo. But it would be as if one ſhould ſay, that after their former organs were entirely or partially changed, they were new beings. This would rather be a creation, than a change.

The ſeminal vermiculi not only differ from animalcula in ſhape; I have likewiſe ſhewn them to be of a nature and conſtitution eſſentially different. Such are the differences mentioned in the former chapter, and in that preceding it; and I omit them here, to avoid repetition.

M. Bonnet ſuggeſts an ingenious experiment: to try whether the ſeminal vermiculi would live in infuſions, and the animalcula of infuſions in ſeminal fluids. I had already done this in part; but upon paſſing the animalcula of putrid into recent ſemen, and the vermiculi of recent ſemen into that which was putrid, I ſaw the whole vermiculi and animalcula periſh. At the ſame time, to ſatisfy the curioſity of my illuſtrious friend, inſtead of corrupted ſemen, I made uſe of vegetable infuſions. I took the precaution, that thoſe where I put the vermiculi were of the ſame heat as the ſemen when

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in the body of the animal; and that the femen wherein the animalcula were put, fhould have the temperature of the atmofphere: but I never could by this method prevent their death. There was, however, a little difference: the feminal vermiculi perifhed immediately, and the infufion animalcula died in a few minutes.

The very great difference of aliment to which the vermiculi muft be accuftomed, upon paffing from without to the femen of animals, feems to me a fufficient reafon why that cannot be their origin. For, the fmalleft animals of our world perifh when obliged to change their food, as we fee in caterpillars feeding upon determinate plants; when the plants are changed, they die. So that, if we give filk-worms any other than mulberry leaves, they very foon fuffer, if they do not immediately die. Befides, not only do infects perifh when the plants are changed, but alfo when the places of the fame plant they naturally occupy are changed. How many hundred fpecies of infects feed upon the pear tree? The ligneous parts ferve fome for food and habitation: fome infinuate themfelves between the bark and the wood, and never quit their retreats: others, to conceal themfelves, fold feveral leaves together, and feed upon the moft delicate parts: others prefer the branches, and, penetrating, form tumours there: fome menace the flowers, and others the fruit. Let us change

change this order: let us tranſport the inſects of the wood into the bark, and, reciprocally, thoſe of the leaves to the roots, and ſo on with the reſt, changing their nutriment and abode. Doubtleſs, we ſhall ſee them quickly periſh. I cannot perceive why the ſame thing ſhould not happen to ſpermatic vermiculi, if they paſſed from water to the ſemen, ſince their aliment is completely changed. It is not enough to ſay, that we have an example in the worms in the horſe, the ſheep, or the ſtag, which live in a place, however, where their exiſtence did not begin; for I may reply, that they have not paſſed from a large to a little world, after living in the former, but are developed in the quadrupeds where they have been depoſited by flies, where they remain until maturity, and where they feed upon the ſubſtance of the animal. If, before they acquired this maturity, their ſituation was fortuitouſly changed, it is moſt likely they would periſh. We ſhould ſee them periſh, or rather, not expand, if the producing flies laid their eggs elſewhere. Whence it follows, that this example confirms the general law.

But if we cannot believe that the vermiculi come from without, what is their origin? We ſhall reply, what Valliſnieri has ſaid upon the origin of the worms of the human body: They are produced, fed, and they multiply in us and

and in other animals: they paſs from generation to generation with the nutriment the mother affords in the uterus, and the milk which is imbibed. This hypotheſis ſeems to me more likely than the other. According to M. De Buffon's aſſertion, the ſemen of the female is full of vermiculi perfectly like thoſe in the ſemen of the male. I doubt not the truth of this: it has been obſerved before him, and deſcribed by Signor Bono, a celebrated phyſician, and an excellent obſerver of ſpermatic vermiculi, incapable of altering any fact, as he is unprejudiced in favour of theory [a]. What has been obſerved in the female ſemen by thoſe two naturaliſts, I have ſometimes, but rarely, ſeen in the blood. In my long reſearches upon the phænomena of circulation, I happened to obſerve in the meſenteric blood of a frog, and three newts, I know not how many of the ſeminal vermiculi peculiar to thoſe amphibia. There was no hazard of being deceived, becauſe there was no room for miſtake. One could not ſay that thoſe vermiculi fortuitouſly mixed with the blood, from the rupture of ſome of the blood veſſels of the teſticles or the vaſa deferentia, ſince the frog and two of the newts were females, and the blood veſſels, as well as the generative organs of the male, were entire, as I aſſured myſelf by a careful examination,

[a] Vallifnieri, tom. 2. edit. in folio.

mination. The vermiculi were actually imprisoned in the vessels, and were pretty vivacious. They were seen in the arteries, excepting a single time that I saw them in a vein. The artery of a frog tadpole shewed me some again; I even saw some in the blood of a sucking calf, and also some in the red globules of the blood of a sheep. I recognised them as real seminal vermiculi, for they had all their characterstics. Those observations prevented my surprise, when mixing a drop of semen with a drop of blood, so that the vermiculi were forced from their native place, they still lived as before. I have observed the same phænomenon with saliva; and it is natural to suppose that it will be found in other animal fluids.

From these facts I draw two conclusions. The first, that it is not absurd to suppose, that the mother may serve as a conveyance for the spermatic vermiculi, to pass them to the young. The second, that they live in the fluids of the young, particularly in the blood, and are *in a manner* retained there until puberty; then, the semen matured, affords them aliment, and a habitation fit for their propagation. I say, *in a manner;* because the rareness of the seminal vermiculi in the blood, sufficiently proves it not to be a fluid, which agrees too well with them, happening perhaps from the aliment found

there

there not being very fit for them. Although the ſemen is derived from the blood, yet are they two very different fluids. To this it may be oppoſed, that the vermiculi found in the blood of the male are the ſeminal vermiculi, re-abſorbed by the veſſels, and mixed with the maſs of the blood. This objection would be well founded in adult males; but it is inſufficient where they are not ſo, as in tadpoles and ſucking calves: in the former, the organs of generation are not developed, and thoſe of the latter are not inhabited by vermiculi.

The father may be an inſtrumental cauſe of the propagation of the vermiculi. This may take place in almoſt every ſpecies of animals. This method I find at leaſt more direct, or perhaps more natural, than the other: I ſpeak of the act of fecundation, which may convey the germs of the vermiculi to the embryo, by the immediate vehicle of the ſemen. That the eggs of females may be fecundated, they muſt be bedewed by the ſpermatic fluid of the male, which muſt act upon the included embryo: it ſhould act not only externally, but alſo internally, for we know that it regulates the parts of the fœtus [a]. It is therefore neceſſary that the fœtus ſhould be penetrated by it; and in this manner it will be eaſy to introduce the vermicular germs. Theſe germs expand, and lay the

[a] Bonnet, Preface de la Contemplation.

the foundation of a little colony of vermiculi; which takes poſſeſſion of the ſeminal fluid, and gives birth to a numerous people.

The ſecond queſtion reſpects the manner in which the vermiculi propagate. In the almoſt infinite number of experiments which I have made, I have always paid attention to this intereſting point. After ſeeing a prodigious number of infuſion animalcula multiply by a diviſion of the body, I inveſtigated whether the ſeminal vermiculi propagated in the ſame manner. But I have had no indication of this. It is true, when they proceed from the animal's body, or when the vermiculi begin to be ill, they are leſs fit for dividing than when in their natural ſtate, when they are vigorous and full of life. I do not deny, that this is poſſible; but, ſay it was ſo, it ſeems morally impoſſible that among ſo many millions of vermiculi which I have at different times obſerved, among ſo many ſpecies, there was not one in a ſtate to divide, or which did divide, as is obſerved with the animalcula of infuſions: neither have I obſerved, that the vermiculi propagate by ſhoots, like polypi. So that, abiding by the different modes of propagation hitherto known, it would ſeem a proper concluſion, that the vermiculi multiply by means of a fœtus, or of eggs: but I muſt admit that I never ſaw either the one or the other.

I

I arrive at last at the question; What is the use of the vermiculi? Lewenhoek's opinion is well known; he thinks they are the immediate authors of generation: so that those of man will be so many homunculi, and those of horses so many foals, and thus of the rest. We cannot deny, that this idea is very ingenious: it is unfortunate, that it is not real. I should depart from my plan, did I attempt to refute it: authors celebrated, and known to philosophers, have done this with success. But I cannot resist saying a word upon the beautiful discovery of Haller, which is completely decisive. He proves by facts, so convincing that we cannot refuse our assent, that the fœtus belongs to the female; that, entire, it pre-exists fecundation. It is therefore clear, that the male vermiculi cannot be fœtus. The circumstances establishing this discovery, are explained in his treatise upon the chicken.

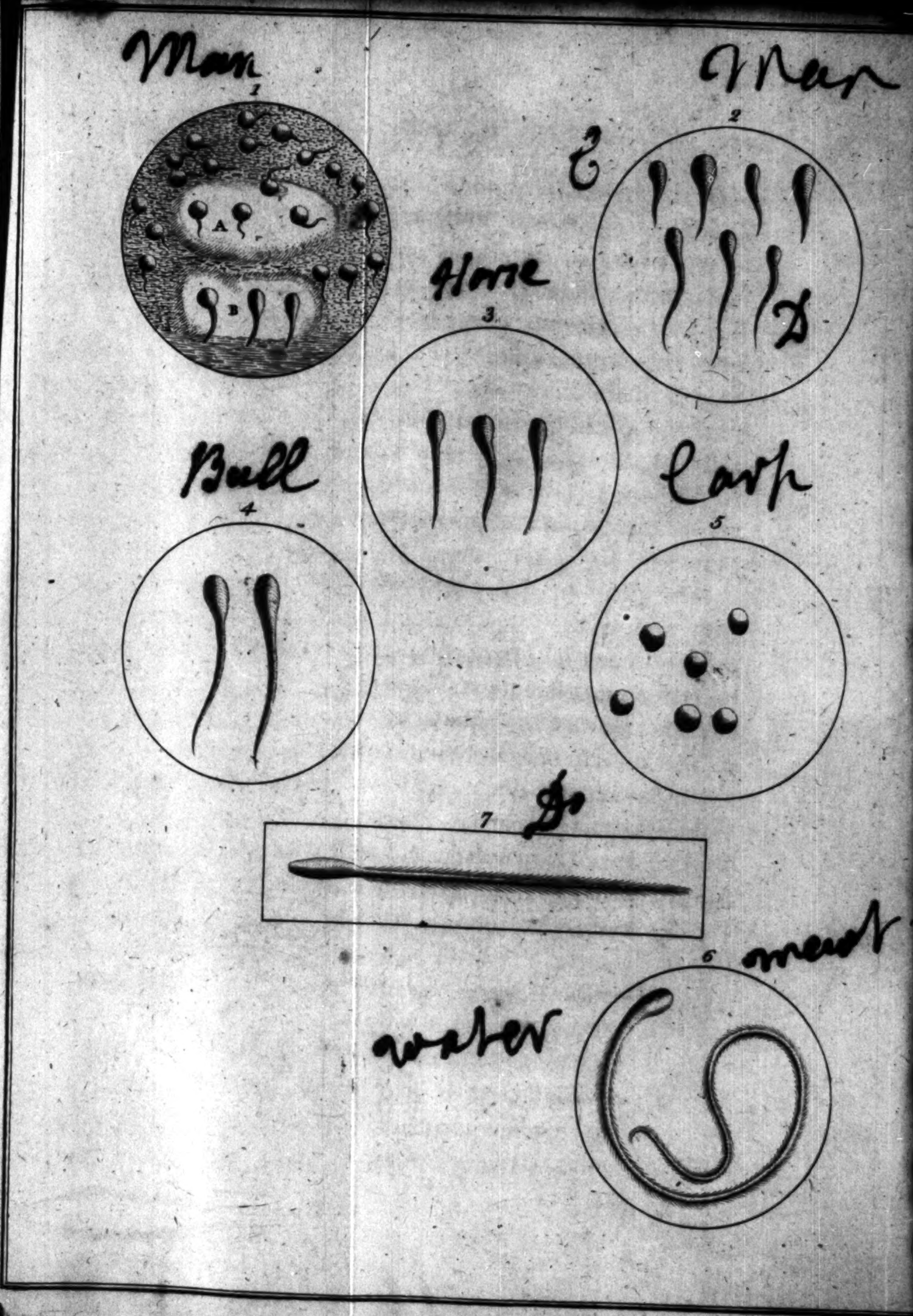
If we refuse the vermiculi this noble purpose, we do not fail to ascribe others to them. Some physiologists have thought them the cause of venereal pleasures: others, that they preserve the fluidity of the semen: and others, that they effect certain unknown purposes. All have endeavoured to divine it. There exists in us and in other animals, this wonderful multitude of animated beings, and many larger vermes, of which Rhedi has given a history, in a work

upon the ſubject, beginning with man, and deſcending to all animals, not excepting the ſmalleſt [a]. Each has externally animals which it feeds, as this naturaliſt proves in the ſame work. But why do all thoſe generations of animals inhabit the external and internal parts of others; why were they created? This I think is beyond the ſphere of human knowledge; and it will not diſpleaſe, if here I ſhould be ſilent. And I hope I ſhall be pardoned for only gueſſing at the other two: their intricacy has prevented me from doing more.

The intent of this work, was to examine, with all poſſible attention and accuracy, the properties and nature of the myſterious inhabitants of animal ſemina; allowing myſelf to diſcuſs and refute the opinions of others learned in the ſubject, becauſe their celebrity and diviſion had created doubt. I leave the learned, judicious, and impartial reader to conſider, whether I have ſucceeded, and whether I have diſſipated, or at leaſt diminiſhed, the clouds which veiled them from the truth.

[a] Degli animali viventi, negli animali viventi.

EXPE-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EXPERIMENTS AND OBSERVATIONS

UPON

ANIMALS AND VEGETABLES CONFINED IN STAGNANT AIR.

INTRODUCTION.

If the privation of air is a powerful method of preventing the production of animated beings, and of destroying life in those already in existence; the presence of air itself is consider-ed equally noxious, when its circulation is impeded. A general rule has been established, That all animals and vegetables, forced to respire the air of close vessels, perish irrecoverably. It is thought equally certain, that seeds do not germinate, and that eggs are not excluded in the same situation. The great Boerhaave speaks thus of all. "Ovula quorumcumque insectorum in vitris accurate clausis "non producunt, licet tepore fota foetus se-"mina plantarum vitæ macerata, optimæ com-"missa terræ atque requisito excitata calore,

"non

" non tamen crefcunt, neque dant vitæ ulla
" figna actuofæ."

Such is the opinion of all philofophers and naturalifts, which for feveral years I confidently adopted, which I believed certain with refpect to the animal and vegetable kingdoms. But my experiments upon infufions, infpired me with a juft diftruft of it. I had difcovered, that the animalcula of infufions were produced and lived in veffels hermetically fealed; and I had feen the feeds I made ufe of, germinate there. Thofe facts did not agree with the opinion generally received, which induced me to inveftigate the limits and conditions of the maxim, *That air which is confined, and cannot be renewed, is noxious to the beings in life, whether animals or vegetables, fubjected to its influence.* With this defign, I refolved to repeat and to diverfify my experiments upon the animalcula of infufions, and the feeds of plants growing in clofe veffels. I likewife wifhed to make experiments upon fome animals, the great analogy of which with the animalcula of infufions, would induce us to fuppofe, that the noxious influence of ftagnant air would not fo eafily affect them, as it would animals ranking higher in the fcale of beings. I therefore made experiments upon the eggs of many terreftrial and aquatic infects; and nature has afforded thofe

illuftrations,

illustrations, which former authors have sought in vain.

From effects I ascended to causes, and enquired why confined air could, in certain situations, be injurious to animated beings. Thus I passed from one research to another. The work insensibly encreased in my hands, and became much more considerable than I at first expected. Since I presume to publish it; to lessen the tædium of the reader, it is divided into three chapters or sections. The two last, in particular, explain the cause of the death of animals; and the first takes a comprehensive view of those which are subject to the impression of stagnant air only in certain circumstances.

CHAP. I.

I PROVIDED a certain number of vessels. In each I put an infusion of vegetable seeds, and then sealed them hermetically. I had got them expressly made at the glass-house: they were large, each would contain 14 or 15 pounds of water [a]. To examine the infusions, it was not necessary to break the vessels; but it sufficed to cause the included liquid pass to a dry part of the glass, and then observe it with a powerful magnifier. The glass was so transparent, that the animalcula were seen swimming in the fine pellicle adhering to the internal surface of the vase.

My experiments were made towards the end of Spring; and it was not long before animalcula begun to inhabit the vessels, and to inhabit the whole. The periods of their encrease, diminution, and destruction, were the same here, as in the open air.

Upon repeating the same experiment several times with different seeds, all afforded animalcula. I remarked one difference; the number of animalcula was never so prodigiously great, which

[a] Twelve ounces in the pound.

which I have obſerved before. One mode by which thoſe animals propagate, is the natural diviſion of the body; and this diviſion likewiſe takes place in confined air. Applying the magnifier to the ſide of the veſſel, I ſometimes ſaw the animalcula dividing through the middle, ſo that the parts were connected by a ſhort filament: others appeared like two minute elongated ſpheres, touching in ſeveral points: others exhibited, on the outline of the body, a contraction, or the rudiments of a diviſion hardly begun.

The duration of life, and the multiplication I had obſerved in the animalcula of infuſions, I likewiſe ſaw in the anguillæ of vinegar confined in cloſe veſſels. From the beginning of April, till the end of November, they were ſeen; and they continually became more numerous. It is true, as the Winter advanced, the eels periſhed; but the ſame happened to thoſe in vinegar in the open air, the cauſe of which was the encreaſed cold. We know that, during Winter, vinegar is without eels.

While I made thoſe experiments, the water of ſome ditches was full of worms, inſects, and the tadpoles of frogs. Upon theſe, I made the ſame experiments I had made upon the animalcula of infuſions, and the eels of vinegar. I begun with the larvæ of inſects. Several I confined in veſſels with ditch water,

that they might find aliment among the quantity of heterogeneous matter, of which the water was full. The larvæ did not ſuffer from this confinement: all changed to nymphs, from which, in time, proceeded winged flies.

Several tadpoles were alſo confined in the veſſels along with a ſufficient quantity of water, wherein I had put the marſh lentil to feed them. During twenty-four days their ſize conſiderably encreaſed; and they died, it is probable, leſs from the ſtagnation of the air, than becauſe the lentil was completely conſumed. The tadpoles were young. I repeated the experiment upon grown tadpoles, whoſe limbs had already begun to appear. This period I choſe, to learn whether they underwent their metamorphoſis in confined air. Several did undergo it, and diveſted themſelves of the tadpole form, to aſſume that of the frog. But others periſhed before attaining their new ſtate.

Thoſe animals inhabiting the waters, are not under the ſame neceſſity of reſpiring air continually renewed, as the animals which nature has deſtined to live amidſt the air itſelf: I therefore thought of making ſome experiments upon them. But, as I had made experiments upon tadpoles, that is, upon a ſpecies of animal changing its ſtate, I was deſirous to repeat them upon ſome other animals of the ſame nature. Caterpillars firſt ſuggeſted themſelves;

and

and my firſt experiments were made upon ſilk-worms. Thoſe I uſed with this intention, would a few days after have begun to ſpin their webs. Sealing them in a veſſel hermetically, they were fixed upon the leaves of a mulberry branch, the ligneous extremity of which I had immerſed in water, that the verdure of the leaves might be preſerved for ſome time, and ſerve as food to the worms. More than a third periſhed; but the reſt proceeded, as uſual, till the eleventh day: they worked at the webs as uſual, fixed them to the ſide of the glaſs, and there ſhut themſelves up. Butterflies came from nine webs; from two there came none. Upon examining the ſterile webs, I found the larva transformed to a chryſalis; but the butterfly did not come out, as it died while in this form. The webs of the eleven butterflies changing to caterpillars in this cloſe veſſel, were of good ſilk; and their only difference from others, was in not being ſo hard and elaſtic as webs generally are.

Other caterpillars, eſpecially thoſe of the elm and the oak, underwent the ſame changes as the ſilk-worms did in cloſe veſſels. I purſued the ſame method, of confining them in veſſels where I had put ſome branches of the trees, immerſing the lower extremity in water.

The metamorphoſes of the larvæ of large fleſh-flies, were more diſtinctly ſeen. I put a

piece

piece of fleſh, nearly putrid, at the bottom of one of the veſſels: it ſerved them for food nine days, that is, all the time they were worms, and until they became nymphs. We know, that when their change to nymphs approaches, they abandon the maſs of putrid fleſh, and, ſeeking a dry ſituation, moſt commonly conceal themſelves under an arid duſty earth.

Thoſe I had confined, likewiſe abandoned the fleſh, traverſed the veſſel, and were in conſtant motion more than half a day. Their anxiety to eſcape, was clearly evinced; but being unable to effect it, they retired to the ſides at the extremity of the neck, which was almoſt parallel with the table, and became perfectly tranquil. There they inſenſibly contracted; their ſhape and colour diſappeared; they aſſumed a light cheſnut colour, and exhibited every ſign of being changed to nymphs. In this ſtate they remained fourteen days; then the ſhell of the nymphs burſting, the flies, completely reſembling the parent fly, eſcaped. The winged inſects lived ſeveral days in their priſon, and then died, apparently for want of food.

I may ſay a few words concerning what happened to the ſeeds I uſed for infuſions. They developed like the animalcula, all germinated well, and in a few days the budding of the branches and the leaves filled the capacity of my veſſels. I ſhould not neglect to obſerve, that

that all these vegetations appeared to be diseased, whether from their decay before producing fruit, or because their colour was yellowish. But, suspecting that this disease was not so much the effect of the privation of the air, as of the beneficent influence of the sun, and the moisture they must require to be supplied by the roots, which could not be obtained from the small quantity of water, I endeavoured to discover the real cause. I put the same quantity of seeds into vessels hermetically sealed; and, instead of water, substituted a given portion of earth well moistened. The plants soon sprung up; and being some hours of the day exposed in the beams of the sun, in a short time they reached the summit of the vessel, without yellowing, and they did become yellow only after a considerable period. I should add, that the seeds I made use of were, pease, maize, kidney-beans, red and bearded wheat, and rye. I planted two stalks of rye under the neck of the vessel, where there was a sufficient space for their elevation: the neck being very long, they exhibited unequivocal marks of vegetation, and the ear shot out of the calyx. This fructification would have made the greatest progress, but for the approach of Winter.

To complete what I have said of seeds confined in vessels, I shall remark, in passing, that although I have in this way observed more than twenty

twenty ſpecies at different times, I have not found one that did not germinate. We muſt not neglect a fact eſſential for their production. Whenever they ſpring in water, which takes place as well in cloſe as in open veſſels, the infuſed ſeeds muſt be partially above the water, otherwiſe they periſh. This precaution was before me diſcovered by the celebrated naturaliſt M. Duhamel.

But if vegetable ſemina germinate without exception in confined air, what are we to think of animal ſemina and the eggs of inſects, which, according to Boerhaave's opinion, and that of many philoſophers, ſhould remain ſterile, even when the operation of circumſtances the moſt favourable to their production concurs? With reſpect to this, I thought it was better to conſult nature, than to truſt to the ſcience of others. I therefore made experiments upon many eggs. Thoſe I made uſe of, were the eggs of ſeveral ſpecies of ſmall beetles, flies, large fleſh-flies, nocturnal and diurnal butterflies; and I took care to mark ſcrupulouſly what happened to each kind. I foreſee the anxiety the reader muſt experience to learn the reſult of thoſe experiments; and, in two words, I ſatisfy his curioſity by ſaying, the whole different ſpecies were produced the ſame in confined as in open air. Boerhaave, adopting the received maxim concerning the ſterility of eggs in confined air,

thus

thus expresses himself in the *Prælectiones Academicæ*[a]. "Ova Bombycis in aere calido ex-"cluduntur si libere admittatur. Eadem in "phiala clausa nunquam producunt suum ani-"mal." Now, the fact is, that those eggs hatch very well in close vessels, as I have been convinced by every experiment I made.

We must conclude, from all I have said, that the air of close vessels is not an impediment to the production of plants or animals; but that plants, without any exception known to me, grow well there; that animals encrease and propagate their species. Those which undergo metamorphosis, experience the whole successively, in close, as well as in open vessels.

Why do we generally believe that stagnant air injures the production of animals and vegetables? Analogy is, I believe, the cause of this remarkable error. It was observed, that the animals and vegetables upon which experiments were made, soon perished in close vessels; that seeds and eggs confined there were sterile: and a general conclusion was drawn, that stagnant air was decidedly noxious to both the kinds of living beings.

In the beginning of this chapter, I have said, the vessels I employed were very large; that each might contain fourteen or fifteen pounds of water. With those vessels I obtained the results

[a] Tom. 2.

results I have mentioned. But the consequences were very different when I used vessels successively smaller. In proportion as their capacity decreases, the eggs or seeds do not germinate, or they perish when scarcely developed. Animals of every species in a short time die there. All the naturalists who have seen in their experiments a different result from that afforded by mine, have undoubtedly made use of vessels too small, otherwise they would certainly have had the same consequences with myself. However, I do not deny that their error respecting animals, might arise from the nature of the animals upon which their experiments were made; for those animals must inevitably have perished, however capacious the vessel might be; for example, warm-blooded animals. To understand this better, and to evince the truth of what I say, I shall enter upon some details.

The animalcula of infusions originate, live, and propagate, in the vessels I first spoke of: this also happens in vessels one third of the size; only we then begin to perceive the disadvantages of the stagnant air. When the capacity is such as to contain three pounds and a half of water, the number of animalcula is less; they multiply less, and die sooner. Upon diminishing the vessels, the larger animalcula are not seen; and neither large nor small, if the

the internal capacity does not exceed ſeven or eight inches.

The nymph of the gnat ſeems to ſupport this ſituation better than the animalcula of infuſions. In five inches of air, ſeveral changed to the winged ſtate. As the quantity leſſens, they periſh proportionally ſooner.

The eels of white vinegar are particularly remarkable: they live and multiply prodigiouſly in a volume of air not exceeding three inches; and die in ſeveral days, only when confined in a tube where there is an inch vacuum. I ſpeak of white vinegar, for the effects are very different in red. In my experiments, the eels of this did not live five days in a veſſel where the vacuum was eleven inches. This did not happen becauſe the vinegar underwent an alteration in the veſſels, but rather becauſe the eels of red vinegar are of a nature different from thoſe of white; which I believe to be the more probable cauſe of the difference I remarked in the figure of each ſpecies.

Tadpoles periſhed in a very few days in nine inches of air; and in three hours, if the vacuum was only three inches.

When caterpillars and the larvæ of flies are confined in but eleven inches of air, they die before transforming to chryſalids. The larvæ, in particular, ſoon after being confined in a cloſe veſſel, deſerted the putrid fleſh put in along

long with them for food, and with agitation traverſed the veſſel, diſregarding the fleſh. They loſt motion and life after various times, longer or ſhorter. If the veſſel was larger, they lived longer: if ſmaller, they died ſooner.

Larvæ, when changed to nymphs, ſuffered leſs from the ſmall quantity of air. In a veſſel where larvæ had died, I confined the ſame number of their nymphs. The flies of ſome came out: but it is neceſſary to obſerve, that the wings and body were diſtorted: they ſeemed to have been produced againſt the will of nature. This never happened to the butterflies of ſeveral ſpecies of chryſalids, although the vacuum of the veſſel was very ſmall.

What I have hitherto ſaid, will apply to ſeeds and eggs. I omit telling the reader, the trouble I had to find the ſucceſſive capacities of the veſſels where the ſeeds and eggs ceaſed to germinate: but, adopting the general reſult, I ſhall ſay, that when the capacity of the veſſels was but three or four inches, neither ſeeds nor eggs have developed.

We muſt therefore conclude, that the production of vegetables, and of ſome animals, takes place, as well in confined, as in open air, provided the quantity of air in the veſſels is conſiderable; but, on the contrary, when it is not, that it becomes fatal to both. The preciſe

cise quantity which may be noxious, can only be determined by the nature, conſtitution, and quality, of the animals and vegetables confined.

My experiments being made at different ſeaſons of the year, I diſcovered another fact, which is; That the death of animals is not only accelerated by diminiſhing the ſize of the veſſels, as I have demonſtrated, but alſo by the encreaſe of heat. This is particularly ſeen in thoſe animals, which are eaſily procured at any time of the year, and live long without food: ſuch are, newts, leeches, land and water ſerpents, vipers, and ſome ſpecies of fiſhes. I obſerved, as much as poſſible, that the individuals I took for my experiments ſhould be of equal ſize, and equally vigorous; ſo that the compariſons might be more juſt. Here follow the facts which I found. Upon the fifth of April, along with other things, I prepared three jars; the firſt might contain ſix pounds of water, the ſecond four, and the third two. In each, four newts were confined. My experiments led me to examine, whether the animals died ſooner as the volume of air was diminiſhed. This was the caſe.

In the ſmalleſt veſſel, the four newts periſhed in forty-one hours; in the intermediate veſſel, in two days; and in the largeſt, in ſeven days.

Upon the ſame day of April, I made a ſimilar experiment with leeches. I confined four in each veſſel. They lived there a long time, in compariſon to the newts. In the ſmalleſt veſſel, they periſhed in three days; in the next, in nine; and in the largeſt, in thirty-two days.

I repeated this experiment 12. May, in the ſame veſſels, and with the two ſpecies of animals. Both died much ſooner. In twenty-ſeven hours, the newts in the ſmalleſt veſſel died; in the next in three days, and in the largeſt in four days. The leeches in the ſmalleſt veſſel died in two days, in the next in five, and in the largeſt in nine. The more ſudden death of the newts and leeches in the month of May, I ſuſpected to be occaſioned by the encreaſed heat of the ſeaſon; ſeeing, in the month of April, during the greateſt heat, the thermometer aſcended to 57°, while in May it ſtood at 70°. My ſuſpicions were realiſed, ſince, in the months of June and July, the death of the animals was accelerated. In July, the thermometer being at 82°, the four newts in the largeſt veſſel died in twenty-three hours, and the leeches in thirty-five.

What has been ſaid of leeches and newts, ſhould apply to ſnakes, vipers, and fiſhes. The reſults from theſe, correſponded with the former. The death of the whole was not only accelerated

accelerated in proportion to the ſmall quantity of air they were forced to reſpire, but alſo in proportion to the encreaſe of heat: I obſerved the reverſe only twice, which we muſt view as ariſing from ſome accidental circumſtance.

I waited until Winter, to make the experiment inverſely; that is, to learn whether the death of animals was retarded in proportion to the encreaſe of cold. This experiment ſucceeded with vipers and newts, which were the two ſpecies I then had at command.

The newts in the ſmalleſt veſſel lived twenty-two days, in the middle ſized thirty-four days, and in the largeſt two months. Vipers lived ſtill longer. The veſſels were placed in a ſituation where the thermometer ſtood at 48°.

In a greater degree of cold their life was protracted ſtill longer. The newts and vipers did not periſh in three months, when the ſmalleſt veſſels were kept under ſnow. Both kinds died upon being expoſed to the temperature of the atmoſphere for ſome days during Spring.

Such are the principal reſults I have been able to collect, from the experiments related in this chapter. Thoſe reſults are of great utility, becauſe they elucidate the ſubject; but they leave us at liberty to fancy the cauſe, or rather the deſire of ſeeking it. If the obſerver is a philoſopher, he will endeavour to diſcover the

reason accelerating the death of animals in small vessels, and retarding it in large. Why is it accelerated more by heat than by cold? Whence arise those diversities in the time of an animal's death? Why may one quantity of air be noxious to one species of animals, and indifferent to another? The solution of these problems depends upon our knowledge of the cause of death in stagnant air. This ancient and most famous question has always divided the celebrated modern philosophers. Notwithstanding its importance, I shall attempt to discuss it. I shall examine what has been already written upon it, and shall adopt the opinion which to me seems the most consistent with facts; that is, with truth. Since we have seen that the eggs of animals, and the seeds of plants, remain sterile when put in a small quantity of air, I shall not fail to add a short sentence or two upon the cause of their sterility.

CHAP.

CHAP. II.

THOSE who have killed animals in close veſſels, have, in their experiments, remarked two phænomena: firſt, that there is accumulated upon the ſides of the veſſel a quantity of vapour exhaled from the animal; ſecondly, that the air has loſt a certain degree of its elaſticity. Theſe two phænomena have produced different opinions. By one, the death of the animals is aſcribed to thoſe exhalations which, being confined in the veſſels, are reſpired by the animals, and thus become fatal. Another opinion maintains, that the exhalations cannot be mortal; but that the diminution of the elaſticity of the air, occaſioned by the exhalations, or a portion of the air being deſtroyed by reſpiration, becomes fatal to the animals.

The experiment of Piſtorini of Bologna is ſpecious. To appretiate the force of both opinions, he reaſons thus. Suppoſing both opinions to be juſt, it ſhould neceſſarily happen, that two animals confined in the ſame veſſel die ſooner than if they were alone, provided the veſſel is the ſame, and the animals of the ſame ſize and ſpecies. We muſt therefore recur to the exhalations from the animal, or the dimi-

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nished elasticity of the air, occasioned by the effluvia escaping from its body, or by the respiration itself. But it is always certain that, doubling the number of animals, the exhalation and respiration are doubled; and consequently the diminution of the elasticity of the air should be doubled. Pistorini did not find this result. Two animals died in the same time as one; although he used the same vessel, and animals of the same size and species *.

The singular consequences of the experiment, induced others to repeat it, among whom was the illustrious Professor Verati. His experiments were made upon birds, and upon frogs. A pigeon lived three hours and three quarters. Two pigeons in the same vessel lived only half the time. Three swallows died in little more than half an hour, two swallows in less than an hour, and one swallow lived almost two hours. He remarked nearly the same with sparrows and quails: three died sooner than two, and two sooner than one. But with frogs it was quite different. In eight days four died as soon as two, and one alone lived no longer than three. So that the experiments upon birds were very different from those of Pistorini; and those of Pistorini agreed with the experiments Signor Verati made upon frogs. And here we see, that, in this mode of death, nature is different

* Act. Bononien. tom. 2. part. 1.

ferent in different animals, which is the cause of the discrepancies between Verati and Pistorini.

Signor Cigna, a celebrated professor, has also engaged in an examination of those differences: he thinks to have destroyed them by his exact experiments; the result of which is this. Where the frogs confined in the vessels are deprived of water, as it would appear those of Signor Verati have been, there, it is true, that the plurality of frogs does not accelerate the death of those confined; at least, this is often the case. When frogs are confined along with water, which is their natural aliment, it is almost certain that the acceleration of death is in proportion to the number of those amphibia [a].

The experiments related in the preceding chapter, induced me to engage in this enquiry also. If it was true, that several animals of a given species confined in the same vessel, died in the same time as when there was only one animal, and if the phænomenon was not accidental, but constant, it must (as I said before) be regulated in a manner proportioned to the smallest quantity of air confined along with the animals. But we may easily see, that the smallest quantity of air should always be where there are most animals. As frogs, according to Signor

[a] Miscell. Taurin. tom. 2.

nor Verati, had disturbed the order established by this law, and as I could not repeat the experiments of Pistorini, because he has not specified what animals he employed, I made my experiments upon frogs also; some in vessels with water, and some in vessels without, thus to come near the method followed by those authors. In three vessels, each of which would contain five pounds of water, I hermetically sealed up frogs; that is, two in one, four in another, and eight in the third. In this last, the eight frogs perished in twenty-six hours; in the second, the four frogs perished in one day; and in the first, the two frogs perished in two days.

At the same time, I made a similar experiment with other three vessels, as large as the first, with the same distribution in the number of frogs; so that there was no difference between this experiment and the preceding; only, in it, there was no water in the vessels, and in this four ounces were in each. In two days, none of the frogs were alive in the vessel with eight: in that with four, they lived three days and a half; and five days where there were two frogs. During these experiments, the thermometer stood between 63 and 70°.

I repeated both experiments; the circumstances the same in every respect, excepting that the heat of the weather was greater, and then

then the thermometer aſcended to 90°. In the firſt of theſe experiments, the eight frogs in the firſt veſſel died in twenty hours; the four in the ſecond in nineteen; and the two frogs in the third in about two days. As to the ſecond experiment, made at the ſame time, and in which each veſſel contained about four ounces of water, the reſult was as follows. The eight frogs in the firſt veſſel died in thirty-two hours; the four in the ſecond in two days; and the two in the third in three days and a half.

I repeated both experiments ſeveral times, which, to avoid the tædium of dry details, I ſhall not circumſtantially deſcribe, and will only ſpeak of the reſults which I had from the veſſels without water. Sometimes I obſerved thoſe irregularities which I have already remarked. It ſometimes happened, that a greater number of frogs periſhed in the ſame time, and ſometimes later than a leſſer number. But when the frogs were in water, they conſtantly periſhed ſooner, as their number was greater: eight died firſt, then four, and laſtly two. It happened only once, that all the eight were alive when one of thoſe was dead in the veſſel confining four.

From all theſe facts, added to thoſe related by Signor Cigna, it muſt reſult, that frogs corroborate the general rule, *That animals, without exception, periſh in confined air ſooner, accor-* *ding*

ding as their number is encreaſed. At the ſame time, diſcrepancies will be ſeen in frogs confined in veſſels without water: but I know not whether they ſhould be regarded as ſuch, becauſe the privation of water is injurious to thoſe animals; which is remarked by Signor Cigna. Frogs in open veſſels die in a ſhort time, if they want water. It is therefore abſolutely neceſſary to proſcribe this cauſe diſturbing our experiments.

The reaſon of the diſcrepance in frogs being found, perhaps it would not be ſo difficult to find it in the animals of Piſtorini, had he mentioned the ſpecies, and the manner in which he conducted his experiments. With reſpect to what Signor Verati relates, we only know, in general, that he made uſe of birds. But he has found that this kind of animals, as I have myſelf found, and as I ſhall ſoon obſerve, agrees well with the rule of which we ſpeak. We have therefore reaſon to ſuſpect, that Piſtorini's experiment has met with ſome accident, without knowing in what the accident conſiſted, which rendered his reſults different from thoſe of others. Perhaps it was occaſioned by the birds themſelves: perhaps, that one which he confined alone, was leſs vigorous than thoſe he confined together; whence they all died in the ſame time. Perhaps, in the experiment with the two birds, all communication with the

the external air had not been prevented; which might easily happen, if the top of the vessel had been covered only with leather, or any substance of a similar nature; or if, upon inverting the mouth of the vessel, it had not been well fixed with mastic, or some such substance, to the plane of position. It is very possible that some invisible hole might remain, or some opening, by which the air might get admission to the vessel. To take away all suspicion of foreign air, it is necessary to seal the vessel hermetically, or to immerse the mouth deep in water; as I shall more clearly explain.

In the preceding chapters we have seen, that the heat of the weather accelerates the death of the animals confined. May not we suspect that Pistorini had made the experiment upon a single bird in very warm weather, and that upon the two birds in very cold; and the death of the two birds by this means be retarded, by the coldness of the air, which would occasion the death of both, in the same time as that of the bird which was alone, because the death of this bird might be occasioned by the greater heat? Here I shall leave this irregularity. But, before resuming my principal object, I should throw a glance upon a doubt which occurred upon seeing animals die sooner in a close than in an open place, when the number of them is encreased. I know not whether the more immediate

mediate death is caused by the diminution of the volume of air alone, or if the number of animals will in another manner effect the more immediate death, and become a new cause why it should happen. To be satisfied which was the case, I selected three equal vessels, and a certain number of the largest frogs, as equal in size as possible. I confined two in one vessel, along with a pound of water; and put one alone in each of the other two vessels, along with a pound of water, adding a quantity of water equal to a frog in size. To discover what quantity equalled the bulk of a frog, I immersed it in a vessel, observing the quantity that overflowed. By this method, the quantities of air in the three vessels were equal, although in one vessel there were two frogs, and in each of the other only one. If the greater number of animals occasioned the acceleration of their death absolutely, because the volume of air was diminished; in this case, the bulk of air was equal in all the three vessels, and the four frogs should die in the same time nearly. If the greater number of animals influenced the acceleration of death, the two frogs in the same vessel should die first. I have said nearly, for although every thing is equal, it would be very remarkable if the animals died in the same time. The two frogs in the first vessel lived two days; that in the second vessel died in three

three days and seventeen hours; and that in the third, died in three days and a half. This result will demonstrate the influence of number in accelerating the death of animals; and other experiments confirm it in a manner which cannot admit of contradiction. Five times I have repeated the same experiment, with the same consequences. The two frogs in the first vessel, always died sooner than those in the second and third; and the difference of time was very perceptible; it has uniformly been a day, sometimes a day and a half, and sometimes longer.

I made a variety in the experiment in another manner. Instead of two frogs, I put three in the first vessel, and only one in the rest; but equalizing the volume of air, by adding a quantity of water equal to two frogs in bulk, which made the quantity of air equal in each. The three frogs in the first vessel died not only sooner than those in the other two, but the difference here was still more conspicuous than before; it was two days and seven hours in the death of the frog in the third vessel. Encreasing the number of frogs in the first vessel, their death was constantly accelerated with respect to the frog in the second and third; although I took care to equalize the volume of air, by adding water equal in bulk to the frogs in the first vessel.

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This experiment upon frogs I extended; and changed the subjects; making experiments upon several small terrestrial quadrupedes and birds. But, notwithstanding the equal quantity of air in the vessels, I always found that as the animals were more numerous, they died sooner, and that the acceleration of their death was constantly in proportion to the encrease of the number.

Thus it is evident, that the death of animals in close vessels follows this invariable rule, that it happens sooner as the animals are more numerous. But, by what physical agent; by what means are they in this manner destroyed? Perhaps it is by their breath, or perhaps by the humidity of the air which they respire. Let us examine these two opinions with care; beginning with that which is founded upon an alteration in the state of the air.

That the air loses part of its elasticity, evidently appears by barometers placed in the vessels where animals are confined. The descent varies. Stair observed, that his barometer fell an inch in a vessel where a rat had died. In the experiments of Verati, it sometimes fell eight lines, sometimes nine, twelve and more, according to the nature and number of the animals confined. The experiments of Mayow, Boyle, Hales and others, agree in establishing,

eſtabliſhing, that a portion of the air is deſtroyed by animals confined in a cloſe veſſel.

We cannot from thence conclude, that the death of animals in cloſe veſſels is occaſioned by the diminiſhed elaſticity of the air; at leaſt, there are no experiments poſitively proving it. We muſt firſt ſee whether this alteration of the air always takes place when animals die in cloſe veſſels. Secondly, whether this degree of alteration is ſufficient to kill the animals: for we know that every degree in the diminution of the elaſticity of the air is not fatal to them.

In this Signor Cigna has laboured in a manner worthy of praiſe, and I ſhall make uſe of ſome of his ideas. I have made a courſe of experiments with the ſame intention, which ſhall be abbreviated after mentioning the methods employed. I uſed ſeveral air-pump glaſs receivers. I immerſed them in water, inverted on a plane. They were of that kind opening and cloſing above by means of a ſtopcock. The receiver being left open when immerſed in water, there was a free paſſage left for the internal air to eſcape above, in proportion as it was compreſſed by the riſing water; and thus the remanent portion of air in the receiver preſerved the natural degree of denſity of the external air, which was abſolutely neceſſary for the exactneſs of the experiment. This

This being done, I closed the vessel; and, to make it more secure, I passed several folds of leather around the stop-cock, which took away all communication with the external air: I was certain that the external air could not insinuate itself, for I used the same receivers as in my pneumatic experiments. Having put the animals in the receivers, I could mark the diminution of the elasticity of the air, by the ascent of the water within. If the animals were aquatic or amphibious, I let them remain in the water. If terrestrial, I put them in a vessel which was suspended by a hook at the top of the receiver.

My first experiments were upon frogs. I confined seven in one receiver, leaving a bulk of air equal to a pound of water. In half an hour, the water within began to rise above the level of that without; which evidently proved, the elasticity of the internal air was affected. The ascent continued until all the frogs were dead, or dying. The water had risen eleven lines.

I repeated the experiment in the same manner, confining only four frogs in the receiver. When they were dead, the water had ascended ten lines.

The elevation of the water was one line higher, in an experiment with two frogs. The

The water aſcended nine lines, when I uſed only one frog.

I made theſe experiments upon newts, reſerving in the receiver the ſame quantity of air as for the frogs. The death of eight newts raiſed the water ſcarcely an inch; of four newts, nine lines; of two, ſix lines; and of one, five lines. The elevation of the water, therefore, diminiſhed with the decreaſe of the number of newts.

After the death of eleven leeches, the water aſcended five lines and a half; and after the death of three, only one line.

Several naturaliſts have remarked how much ſmall animals deſtroy the elaſticity of the air. I had remarked this alſo in birds: it was chiefly upon them that the reſearches of Verati were made. But nobody, that I know of, has made experiments upon an animal which partakes both of the nature of a bird and of a quadruped, although it is not properly either the one or the other. I ſpeak of the bat; this animal, ſo diſguſting and forbidding in appearance, but which is at the ſame time as perfect as other animals, and connects quadrupedes with birds. The ambiguity in the nature of thoſe animals, made me wiſh to make them reſpire the ſame air in cloſe veſſels; but I thought firſt of trying how long they could ſupport a vacuum. They died ſooner than cold-blooded ani-

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mals. Five bats, ſucceſſively ſubjected to the experiment, did not live three minutes. They were of that ſpecies, called by M. D'Aubenton, the horſe-ſhoe bat, from the circular line upon the noſe.

Although the death of bats in vacuo was ſudden, it was under limitations. Four bats confined in a cloſe veſſel, lived ſcarcely an hour and a half: one lived almoſt three hours. The water of the veſſel in which were the four, aſcended an inch and ſeven lines; that in the veſſel where there were two, an inch and three lines; and in that with one, eleven lines.

I extended my experiments to ſeveral reptiles, vipers, and ſome land ſnakes. Both ſpecies of animals having died in cloſe veſſels, the water was elevated to a certain degree, as their number was greater. The greateſt elevation occaſioned by three vipers, was an inch and three lines; and the leaſt elevation by the death of one viper, was ſix lines. The death of one ſnake, raiſed the water four lines, and of five ſnakes, an inch and ſeven lines.

There is the ſame law with ſmall lizards and fiſhes. The elevation of the water is proportioned to their numbers.

I have repeated all theſe experiments at all ſeaſons, and have always ſeen the water within the receiver riſe, with this ſingle difference, that the elevation is more accelerated in warm,

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than in cold weather, as well as the death of the animals upon which the experiments are made. I have alſo conſtantly obſerved, that the elevation of the water is ſo much the leſs, as the animals are ſmaller. Vipers, ſnakes, and bats, raiſed the water more than frogs, newts, and lizards; and lizards raiſed it more than leeches. The death of a barbel weighing a pound, raiſed it above an inch; that of one weighing two ounces, raiſed it only two lines. It ſeemed to me, that in conſequence of this proportion, the death of the ſmalleſt animals ſhould raiſe it very little, or hardly at all; which would happen, if their death was not occaſioned by the diminiſhed elaſticity of the air; becauſe, the water within the receivers remaining at its original level, would evince, that the elaſticity of the air underwent no alteration.

To diſcuſs this fact, was of the greateſt importance. I begun with the larvæ of large flies. Thirty were put into a very ſmall receiver. They were extremely minute, being newly produced. I left them upon the fleſh where they had been depoſited by the mother. They lived only ſeven hours in the receiver; and the water aſcended a line and a half. I repeated the ſame experiment upon other fifteen worms. After their death, I could ſcarcely perceive that the water was elevated above the

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level; and it undoubtedly was not, when I repeated the experiment upon eight worms, although the whole died.

The larvæ of common flies exhibited nearly the same phænomenon. The water rose a third of a line, when the number was great; when it was small, the rise was not sensible.

Seven earth worms dying, did not raise the water.

The larvæ and nymphs of gnats, had only an inch of air, and died in less than a day. They were to the number of some hundreds; yet, after their death, the water in the receiver stood at its original level.

The death of five rat-tailed worms confined in a receiver, did not sensibly alter the level of the water; but the death of a greater number occasioned a perceptible elevation.

Some stagnant waters are full of a sort of minute animals, called by naturalists, water-lice or fleas. Those animals are in constant motion, darting through the water where they are. Several thousands lived two days and some hours in a receiver, and died without a sensible elevation of the water.

I could perceive no elevation in water full of animalcula, confined in a receiver, although the whole died in two weeks.

The experiments I made upon several insects undergoing no metamorphosis, as, shell

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and naked ſnails, millepedes, &c. and even upon others which do change, as, caterpillars, chryſalids, and nymphs, demonſtrated to me, that the death of a great number raiſes the water a little in the receiver; when the number is ſmall, there is no elevation.

We have now enough of facts to decide the queſtion in which we have engaged, eſpecially when they are compared with thoſe of Signori Cigna and Verati; and I draw two principal concluſions. Firſt, That there are ſeveral animals, which, dying in cloſe veſſels, do not diminiſh the elaſticity of the air: The ſecond, That thoſe which diminiſh it, do ſo very little. The firſt reſult is unfavourable to the opinion of thoſe, who attribute the death of animals to the diminiſhed elaſticity of the air. We know, that we cannot aſcribe it to this: for if, in many caſes, animals die, without the air loſing its elaſticity, we muſt conclude that their death has another cauſe. The little diminution which the elaſticity of the air ſuffers, makes me doubt much, whether, in the other caſes, it occaſions the animals' death. By Signor Verati's experiments, it appears, that the barometer fell little more than an inch at the greateſt alteration. According to my experiments, the water in the receiver ſometimes roſe a few lines, ſometimes near an inch, and at the greateſt degree an inch and ſeven lines, that is, one

fourteenth of an inch, and ſeven fourteenths of a line of mercury. But we know, that, in the changes of weather, there is a greater difference in the weight of the air. The mercury in the barometer ſometimes falls more than an inch: it falls quickly, eſpecially in ſtorms, without affecting animals: otherwiſe, both the cold and warm-blooded animals would not live in ſafety on mountains, where the mercury falls lower than in the barometers placed in cloſe veſſels. Animals can not only live in air which has loſt its elaſticity, to ſuch a degree that the barometer falls ſome inches, as upon the ſummits of lofty mountains; they can live in air which cannot be renewed, where the barometer falls to leſs than half its natural height. Such is the ingenious experiment of Signor Cigna. This acute obſerver confined two ſparrows in the receiver of an air-pump. One was left at liberty; the other was put into a glaſs veſſel, around the neck of which there was firmly tied a very large Florence flaſk. Then he begun to exhauſt the air, until the mercury within aſcended in the barometer twenty-ſeven inches and a half, and fell without, by the index, to nineteen inches. After this, he let as much air into the receiver, as depreſſed it two inches within. In a little afterwards, he drew the ſame quantity from the receiver, which he ſoon returned; and continu-

ed

ed this alternate exhauſtion and return for half an hour; and the two ſparrows were always kept in air rarefied ſo much as to ſupport eight inches and a half of mercury, or at moſt ten inches and a half. Only the ſparrow at liberty had the benefit of reſpiring renewed air; while that which was confined always reſpired the ſame. This laſt expired, ſoon after being taken from its veſſel; whereas, the other came from the receiver in perfect health. Boyle tells us, that animals periſh in condenſed air rendered denſer than the atmoſphere. I often repeated this experiment, condenſing the air ſometimes twice, and ſometimes thrice, and even more, than the natural air. I have obſerved, with Boyle, that the air become moſt elaſtic, kills the animals ſlower, but that they abſolutely periſh.

It is then demonſtrated, by experiment, that the diminiſhed elaſticity of the air, is not, and cannot be, the efficient cauſe of the death of animals confined in veſſels hermetically ſealed. It remains to ſee, whether their reſpiration contributes to their death; which is the opinion to be diſcuſſed, and will be the ſubject of the following chapter.

CHAP. III.

There are three things to be enquired into, in an examination of the first question, Whether the death of animals in confined air, is occasioned by their respiration? 1. If we actually find exhalations in close vessels where animals have died. 2. If the exhalations are the occasion of their death. 3. Supposing they are, how do they effect it.

I return to the first article. Signor Cigna, who has elucidated this theory, has proved the reality of the influence of the exhalations of respiration, by the fœtid odour we are sensible of upon opening the vessels where animals have died, and by a sort of pellicle of vapour covering the internal surface of the vessels. I shall here relate what I have observed in my experiments. I have almost always seen this pellicle over the internal surface of the vessels where warm-blooded animals had died; such as, birds, rats, bats, &c. But I have never, or almost never, remarked it with cold-blooded animals. I have indeed felt something, upon opening the vessels; and the odour was certainly fœtid and cadaverous. I have felt it in all my experiments, which have been very numerous,

ous, and even in thoſe made with the ſmalleſt animals; ſo that the exiſtence of this vapour is not to be doubted, although it does not always appear within the veſſel, whether becauſe it is not in ſufficient quantity, or whether becauſe it is of a dry nature. Both caſes may prevent it being ſeen under the form of an aqueous veil.

It appears, at the ſame time, inconteſtible, that thoſe exhalations are the real cauſe of the death of the animals. Signor Cigna endeavours to prove it by the reſemblance we remark, between the phænomena ſeen in liquids which evaporate in confined air, and thoſe exhibited by animals reſpiring in cloſe veſſels. The evaporation in cloſe veſſels, as he has obſerved, is proportioned in duration to thoſe two conditions; namely, the ſize of the veſſel, and the rareneſs of the air where it operates. Animals are alſo ſubject to thoſe two conditions. They live ſo much longer as the quantity of confined air is greater, and die ſooner when the air becomes more rare.

Collecting ſome of the reſults which I have hitherto eſtabliſhed, and connecting them with thoſe which I ſhall eſtabliſh hereafter, it ſeems to me eaſy to prove this fact. I have obſerved that the death of animals is accelerated by two circumſtances; by the encreaſed heat of the atmoſphere, and the number of animals confined.

ed. In thoſe caſes, the exhalations are more copious: reaſon perſuades us of this, and experiment confirms it, by the more fœtid odour exhaled upon opening the veſſels. As we cannot aſcribe the death of animals to the diminiſhed elaſticity of the air, and as we ſee no other cauſes, why may not we recur to thoſe exhalations which have become more denſe and active? And further, as we ſhall ſoon ſee, animals die ſooner in cloſe veſſels, where there are others already dead, becauſe there are in theſe more exhalations. We comprehend by this means, how animals confined alone, or in ſmall numbers, live much longer than when there are many; and alſo, why they live much longer in cold weather; becauſe in the two hypotheſes, this leſſens the afflux of the vapours. For this reaſon, the life of animals will be abridged, in proportion to the ſmallneſs of the veſſels. The vapours become denſer, from the little ſpace they have to ſpread.

After all thoſe facts, we cannot ſay, that the cauſe of the death of animals in cloſe veſſels muſt be aſcribed to the diminiſhed elaſticity of the air, inſtead of the exhalations of reſpiration; and if the diminiſhed elaſticity of the air does not contribute to it, ſome great change which the air undergoes, produces this effect: particularly, as the confined air, by the inſpirations and expirations, will ceaſe to be the ſame,

which

which must always happen when a free communication with the external air is interrupted; for then it should lose the *pabulum*, which is a substance or quality known only by name, but upon which we make the life of animals depend; so that the air will be decomposed or corrupted, and assuredly will become unfit for respiration. But the slender support which the opinion, that animals die upon account of the diminished elasticity of the air, can receive, is demonstrated by the animals themselves dying in situations where the communication of the external with the internal air is uninterrupted. When seven frogs had expired in a receiver, the higher part of which was open, and thus the external and internal air preserving an equilibrium: an hour after this, I confined other two frogs, the upper part of the receiver remaining open; but these two frogs died in an hour and a half. I shall further observe, the frogs in the receiver perished sooner, according as the number of dead frogs was greater; although the diameter of the opening above was at least two lines.

Several birds, reptiles, and small quadrupedes, perished when put into a receiver where dead animals were, although the receiver was open above. The constant communication of the external with the internal air, destroys the opinion which ascribes the death of animals to the

the diſorder or corruption of the air. But it remains certain, that death is occaſioned by the exhalations of reſpiration; for I have obſerved that animals die later when the receiver is open above, as ſome part of the exhalations eſcape.

I may remark, in paſſing, that the death of animals, in veſſels where the upper part is open, is an argument which may deciſively prove, that they do not die from the diminiſhed elaſticity of the air.

Enlarging the opening of the receiver, ſo that the exhalations might more abundantly eſcape, I made an experiment, to aſcertain completely, whether they occaſioned the death of animals. I thought of paſſing the exhalations from the receiver into a veſſel applied to the opening, which was eaſily accompliſhed. I confined two ſwallows in the veſſel, after perfectly ſecuring the opening with a wooden ſtopper. Swallows were preferred, as eight had died in the receiver. The influence of the exhalations was ſuch, that eight ſwallows died in a quarter of an hour, although two confined in a ſimilar veſſel lived two hours.

I varied the experiment, by collecting in the veſſel various quantities of the vapours. The life of the animals has always been proportioned in duration, to the quantity of vapour. Long after the vapours have been confined

fined in veſſels, they retain the power of deſtroying animals. It makes little difference in their influence, whatever animals have afforded them: they are equally fatal from all. The exhalations from birds kill quadrupedes, and thoſe from quadrupedes are fatal to birds. It is the ſame with thoſe of fiſhes, with reſpect to reptiles, inſects, and the reſt. To finiſh the proof I had engaged in, I made the following experiment. Several animals having expired in a receiver, during very warm weather, I opened the hole at the upper part of the receiver, and preſented a bird to the very fœtid vapour which eſcaped; ſo that in its inſpiration it was forced to inhale the mephitic air. Every bird treated in this manner died.

Although I believe it impoſſible that any animal can live in confined air, when the veſſels are very ſmall, yet it is certain that ſome may live longer there than others. In general, cold-blooded animals can ſupport this ſituation better than warm. In the ſame air where a newt or a frog will live a day, a ſparrow, a bat, or a rat, will not live an hour. Among cold-blooded animals, there is even a ſort of gradation. A newt lives longer than a frog, and a frog ſhorter than a leech. The ſame is the caſe with a number of inſects. In the opinion of ſome, it is not difficult to aſcribe a reaſon for the variety in ſuch phænomena. Cold-

blooded

blooded animals are not only more tenacious of life, but they have also less necessity to respire, than warm-blooded animals have.

A frog, a viper, a toad, will live longer in vacuo than a bird: those animals should also absorb a smaller quantity of exhalations, from not being obliged to respire so often as the warm-blooded animals. It will not then be surprising, that they live the longer of the two. It is doubtless a reason analogous, which occasions the difference of time that we see a cold and a warm-blooded animal can live in close vessels. Experiment proves that frogs die sooner than newts, as I have myself found, by keeping both immersed in water. We may reason in the same way respecting insects, without recurring to their stronger or weaker constitution, which may likewise produce similar discrepances.

It remains for me to explain, how the exhalations of respiration are noxious to animals; and here, in particular, Signor Cigna has displayed his wonted abilities. The exhalations of respiration kill animals, according to this observer, by irritating the bronchiæ and the lungs, forcing them to contract and become rigid, thus preventing the entry of new air. And, according to this author's opinion, animals in infected air, should die from suffocation. He endeavours to shew, that this is the actual

cause

cauſe of their death, from the different ſymptoms obſerved in the reſpiration of confined animals. When the exhalations begin to collect in the confined air, the reſpiration becomes more frequent and fainter, becauſe the air is ſcarcely inſpired, before the animal is forced immediately to expire it, from the quantity of exhalations it contains. Theſe exhalations continuing to encreaſe, the reſpiration alſo continues frequent, but becomes more laboured. The animal is ſoon exhauſted by this laboured and frequent reſpiration, when confined in air where other animals have died. All this proves, in the opinion of Signor Cigna, that the exhalations injure the animal, becauſe they irritate and make rigid the organs of reſpiration, and obſtruct the entry of the air.

In my numerous experiments, I have ſeen the ſame ſymptoms of altered reſpiration. They manifeſt themſelves in the warm-blooded animals, and particularly in birds. They are not ſo evident in cold-blooded animals; but, at the ſame time, they are ſeen, if we paſs the animals from the open to confined air, which has previouſly been vitiated by exhalations. Signor Cigna had before experienced this in ſeveral animals. He confined a frog in a receiver, where there were five or ſix already dead: the frog inſtantly became agitated, and leapt againſt the ſides of the receiver. The

reſpiration

respiration was at the first frequent and laboured; apparently because it was more painful; and it, in a short time, ended with death. I am, therefore, with the Turin Professor, fully convinced, that the respiration is affected by this air; but I cannot agree, that there happens in animals that contraction of the organs of respiration which kills them by suffocation. When several frogs are confined in a capacious vessel, they live a long time. If the weather is not warm, they retain their vivacity for more than a day; but after this, they become sluggish, and swell excessively. If they are males, the two vesicles upon the sides of the head encrease; and their inflation is such, that they prevent the frogs from sinking in the water; but keep them upon the surface. After remaining some time in this state, the frogs expire. When taken from the vessel and opened, we discover that the swelling proceeds from the inflation of the lungs, which cannot possibly be more distended. The same inflation is found in the lungs of toads and newts dying in the same manner: they also are necessitated to swim. The lungs are so far from being contracted and become rigid, that they are much dilated, and considerably charged with air. For this reason then, and for others of which I shall afterwards speak, I cannot admit that animals die from the impeded respiration. In very

very ſmall veſſels, where ſeveral animals were already dead, I confined ſome vipers and frogs; and at the ſame time I put an equal number of the like animals in vacuo. It is incredible, how much ſooner the former died than the latter. Some of them did not ſurvive a minute; but thoſe in vacuo were alive at the end of ſeveral hours. Therefore, it was not ſuffocation, or an obſtacle to the air entering into the lungs, that occaſioned the death of the former animals, otherwiſe they would have lived much longer, or at leaſt as long as thoſe in vacuo.

At the ſame time, I forced ſome frogs to remain under water, preventing them from riſing to reſpire at the ſurface. In others, I have tied the origin of the lungs in ſuch a manner, that the air could not enter. Others, I have deprived of the lungs, and confined them with other frogs in a ſmall quantity of air become very fœtid by the exhalations of reſpiration. The laſt expired in a few minutes, ſometimes in one, even in leſs; while thoſe deprived of reſpiration, by immerſion in water, having the lungs tied, or cut away, have lived in vacuo for ſeveral hours. I have found the ſame with toads and water-ſerpents. Since the death of all thoſe ſpecies, occaſioned by deprivation of reſpiration, has been, without compariſon, later than the death of the ſame animals killed by the exhalations of their reſpiration, we muſt

conclude, that thoſe exhalations do not occaſion death by the ſuſpenſion of reſpiration; but that they are, with reſpect to thoſe animals, one of the poiſons moſt fit to deſtroy life; and that it acts as ſuddenly as any other poiſon, and even kills almoſt inſtantaneouſly when collected in a great quantity. This poiſon penetrating the body by means of reſpiration, when animals inſpire the air, will cauſe that laboured breathing they experience; for it is more than probable, that it will make, upon the organs of reſpiration, a violent and painful impreſſion. At the ſame time, the veſſels of reſpiration are not the only vehicle to convey the poiſonous vapours to the animal. Earthworms, leeches, and ſome other inſects, which are not only without real lungs, but alſo without ſtigmata or trachea, die in like manner with the reſt in confined air. We muſt ſay then, that the exhalations act upon them, either by inſinuating themſelves through the pores of the ſkin, by the channel of the aliments, or by both. The deleterious quality of the exhalations is ſo formidable to every ſpecies of animal, that it extends to thoſe which never feel the lively impreſſions of the air, from their conſtant abode at the bottom of the water. This induced me to obſerve ſome of the ſnails and ſnakes of ſtagnant waters, put in an open veſſel with water. They crawled over the

the bottom, without betraying any ſign of uneaſineſs; but when confined in a very ſmall veſſel, they were inceſſantly agitated; they aſcended the ſides of the veſſel, contrary to their uſual cuſtom, leaving the water, and ſoon expired. The influence of the exhalations thus acts beneath the waters, which we cannot doubt, from the fœtid odour communicated to the water within the receivers, as well as to the external water, whenever the odour is to a certain degree.

But ſince thoſe peſtilential exhalations do not kill animals by depriving them of reſpiration, how do they occaſion the death of animated beings? It is not by coagulating the fluids, by diſſolving them, or by deſtroying them. Immediately after the death of animals killed in this manner the blood preſerves its original fluidity; the ſerum continues to flow, and the minute globules retain their ſize and figure. Beſides, did the exhalations coagulate the blood, or contribute to make it more fluid, this could not be the cauſe of the ſudden death of animals killed by them: for they could effect it only by interrupting the whole circulation, or depriving the blood itſelf of fluidity [a].

I have ſuſpected that this poiſon kills by deſtroying the irritability of the muſcles. The irritability may be loſt in two ways; the muſ-

[a] De phenomeni della circolazione, tom. 1. c. 6.

cular fibre may either become too relaxed or too rigid. The muſcles do not ſeem relaxed, but rather too rigid, when the animals are taken from the confined place where they have died. Such were the phænomena I obſerved in frogs: their hind legs and thighs were extended in a ſtraight line, as if they had been dried. If their poſition was changed, or if they were bent, a reſiſtance was found, and when at liberty they reſumed their original poſition: the muſcular ſubſtance ſeemed harder under the finger, and under the knife: but I ſoon diſcovered thoſe changes in the muſcular fibre did not precede or attend upon the death of the frogs, but that they followed it. If I took from the veſſel frogs dying, or ſcarcely dead, I did not perceive their limbs extended, and their muſcles preſerved ſufficient flexibility: on the contrary, the extenſion and rigidity only took place when they had continued ſome time in the veſſel. I have obſerved the muſcular hardneſs and the extenſion of the limbs in dead frogs which had remained ſome time immerſed in water. Finally, this change in the muſcular ſyſtem, by no means happens to all animals.

The following facts have made me entirely renounce the idea which cauſed me ſuſpect the loſs of irritability. The muſcles of a frog's thigh diſplay great irritability when cut or

pricked:

pricked: they vibrate and ſuddenly contract, not only where the point of the needle or the edge of the inſtrument touches, but far beyond it; and the vibration continues during ſome time, although the cauſe which occaſions it is ſuſpended. Having taken from a cloſe veſſel a frog upon the point of death, I will not ſay that the muſcles retained their irritability as in a ſtate of health, or that the approach of death did not weaken it; but I will ſay, that the vibrations and contractions of the thighs, when pricked and cut, re-appeared, and even continued after the entire death of the animal.

I conſequently abandoned the idea of the loſs of muſcular irritability: and, after matured reflection, it ſeems to me, that the nervous ſyſtem is the part upon which the exhalations act. And here are the reaſons which have ſuggeſted that opinion. I ſubmit them to the judgement of philoſophic readers. Convulſions commonly precede, and attend the death of our animals. They are clearly ſeen in frogs: the whole body is ſometimes convulſed, but particularly, and more violently, the limbs. In thoſe convulſions they die. As in Winter they longer reſiſt death, the convulſions alſo continue longer. If taken from the veſſel before death, it appears that the ſeat of feeling has ſuffered. They are ſluggiſh, although vio-

lently agitated: they do not change their place, when ſtimulated; the convulſions re-appear; their lethargy augments, even when placed in the open air; and they generally die. Thoſe convulſions are doubtleſs the effect of affected nerves. The reaſon ſeems natural; but here is a convincing proof. I wounded the muſcles of a frog, which had not been expoſed to confined air: the wounding excited in them the motions which ſtimulating ordinarily awakens; but they were never convulſive motions. On the contrary, if I touched the origin of the crural nerves with the ſame inſtrument, which was extremely fine, the limbs ſuddenly became convulſed; and then, in the ſame manner as when in confined air, when I pricked the ſpinal marrow, or the brain, the convulſions became univerſal over the body. I have never been able to ſee the convulſions partial or general in frogs, but by wounding the nerves. It is on this account, that the convulſive ſpaſms in animals confined in cloſe veſſels, make me ſuſpect that the poiſonous vapour acts upon the nervous ſyſtem.

I had ſtill one doubt to ſolve. I could not reconcile the almoſt immediate death of frogs expoſed to the ſtrong exhalations of reſpiration, with the preſervation of their life during a long time, although deprived of the brain. I have ſhewn, in my work upon Circulation, that

that frogs will live ſeveral days, although the brain is taken away. But the doubt diſappeared, when I ſaw their death become ſudden, when, inſtead of wounding the brain, I wounded the ſpinal marrow. If a pin was introduced where the ſpinal marrow unites with the brain, the frog in a moment died convulſed. Animals as tenacious of life as frogs, die with the ſame ſuddenneſs, when the ſpinal marrow is injured. It is not therefore very extraordinary, that death ſo immediate is occaſioned by the peſtilential exhalations inſinuating themſelves in quantities into animated bodies, and, not affecting the other parts, attack the whole nervous ſyſtem, and momentaneouſly deprive it of the ſenſitive faculty.

But, what can we ſay of the death of thoſe animals in cloſe veſſels, if very ſmall, in which there is found no nerve, as in the eels of vinegar, and the multitude of infuſion animalcula? I agree that thoſe moſt minute beings are killed by the exhalations. The analogy of ſo many other animals which periſh from the ſame cauſe, renders this conjecture very plauſible. The exhalations can only deſtroy them by contact. We muſt conſequently ſay, that they act upon the organs of thoſe animals, and there produce an effect ſimilar to that they operate upon the nervous ſubſtance of other animals; although their organic ſtructure may be with-

out nerves, so much at least that we cannot judge of it by the microscope. But, for this reason, they cannot evite the fatal influence of the exhalations; nor do I see how they can escape it, when they cannot resist the effects of the electric vapour, and particularly of certain odours.

Besides, I beseech the reader to view this opinion as a conjecture. I have not made that collection of facts necessary to give it authenticity; but I have not had leisure to enter into all the details, and to make the most profound researches. I wish that others would undertake it; and I shall always have the same regard for those who attain their purpose, whether they confirm or confute my conjecture; for I have no other view but the search of truth.

In the first chapter, I have spoken of eggs and seeds which did not germinate, when confined with a small quantity of air. It is possible, that this sterility may be occasioned by the same causes that destroy animals confined in small vessels. A comparison will help to convince us. Butterflies, as we have seen, chap. 1. do not come from chrysalids confined in small vessels. I find that M. De Reaumur has had the same results, although the object of his experiments was very different from mine. In glass tubes four or five inches long, he hermetically

tically sealed chrysalids. Part of these were from the cabbage caterpillar, and part of that kind transforming to phalenæ. They always remained in their original state, although they had been confined for several months. They never developed; for, as he observed, they perspired; and perspiration is necessary for chrysalids to change to butterflies. These two facts he proved in a decisive manner. When the chrysalids are in a very confined place, as in a tube of several inches, the moisture which transpires cannot dissipate: on the contrary, it falls back upon the chrysalids. Thus, in some days, we see them moist; and this humidity, insinuating itself into their bodies, renders them diseased. Therefore, the death of the chrysalids happens nearly from the same cause, as that of animals in stagnant air. What I say of chrysalids, may be applied to the eggs of insects, and the seeds of plants. We know, that eggs hatch only at a certain degree of heat, which occasions perspiration: if confined in a small vessel, they re-absorb the exhalations which they have before transpired, and then they corrupt. A proof of this, is the humidity which after a certain time covers the eggs, and sometimes, in considerable abundance, the sides of the vessel.

The same happens to vessels confining vegetable seeds. As I have been often wont to put

seeds

ſeeds in a little water, that they might germinate; upon taking them out, I ſaw the part which had been expoſed to the air covered by a humid pellicle.

From oppoſite reaſons, we ſee why eggs and ſeeds are excluded in large cloſe veſſels. They are always in ſafety; for the vacuity being great, the exhalations may diſperſe. In the ſame way, butterflies come from chryſalids, when the capacity of the veſſel is conſiderable.

OBSERVA-

OBSERVATIONS AND EXPERIMENTS

UPON

SOME SINGULAR ANIMALS WHICH MAY BE KILLED AND REVIVED.

CHAP. I.

In the history of the animalcula of infusions, which I have treated so much at large, it has been said, that when once those animals perished from a defect of water or humidity, they could never again be brought to life, although water was supplied, and their immersion continued long. Of this I have had the most convincing and repeated proofs, in the experiments which I proceed to narrate. But there are other animalcula, which, notwithstanding they are the inhabitants of infusions, are so much distinguished and privileged by nature, as to enjoy the advantage of resurrection after death. Such, among others, are the Wheel-animal, the Sloth, the Anguillæ of Tiles, and those of Blighted Corn.

A microſcopic animalcule inhabiting the ſand of tiles and ſewers, is by naturaliſts termed the Wheel-animal. The abdomen is large, and ſituated towards the middle of the body: in the opinion of ſome, there is an heart. The poſterior part of the animal is provided with a minute trident; and the anterior part divides into two trunks, bearing, at the vertices, the ſemblance of two moſt ſingular wheels. From theſe it has obtained the name of the Wheel-animal. One, magnified, is repreſented Pl. 3. fig. 1. If the ſand I have mentioned is put into water, the animalcule exhibits all its organs to the obſerver, provided the ſand remains a certain time infuſed. If the water fails, the action of the wheels, and that of the heart, ceaſes; the animal gradually loſes motion and life; it contracts, becomes very minute, and aſſumes the reſemblance of a dry and emaciated ſkin. To revive it, it is ſufficient to immerſe it in water: then the body ſoon extends, the wheels and the trident appear, the heart is re-animated; motion is regenerated in the whole animal; it begins to ſwim in the water, and exerciſes all the functions of life. It is of no importance that the animal has remained long dried in the ſand. Lewenhoek, the firſt who had the good fortune to diſcover it, and from whoſe works I draw the chief part of what I relate, has ſeen wheel-animals re-animated, after being kept in

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dry ſand almoſt two continued years. With this naturaliſt we muſt obſerve, that when the animal revives, the trunks and the wheels are not always completely diſplayed, but are ſometimes exhibited as in fig. 2. A.

Such are the three figures of the wheel-animal which Baker has, after Lewenhoek, given, in his treatiſe intitled, *The Microſcope made eaſy*. He contents himſelf with repeating only what the Dutch naturaliſt had written before.

Although ſeveral naturaliſts have treated of the wheel-animal, they ſeem to me to have done it but ſuperficially, and all to have proceeded upon the relations of Lewenhoek. I thought it would not miſapply my pains to inveſtigate this intereſting ſubject, and to illuſtrate it by additional facts: and I was particularly induced to this, by the relation between that animalcule and the chief objects of my work. I have therefore compoſed a ſhort and methodical hiſtory of the animal, from the materials with which I have been furniſhed by experiment and obſervation. But, what renders it ſo very remarkable is, the ſingular faculty of reſurrection, poſſeſſed by the wheel-animal. When upon the point of publiſhing the fruit of my labours, another work of Baker, written in Engliſh, fell into my hands [a]: there, much is ſaid of the wheel-animal. I peruſed it rapidly, and

[a] Employment for the Microſcope. London, 1764.

and ſeeing at once, that the author propoſed to treat the matter *ex profeſſo*, I deſigned to ſuppreſs in this work all that concerns the wheel-animal; becauſe it would have been uſeleſs to treat of a ſubject already diſcuſſed by a learned obſerver. I ſhould certainly have done ſo, had I not perceived that Baker's obſervations were materially different from mine; becauſe his wheel-animals had been of a different ſpecies. I therefore reſolved to publiſh my treatiſe, which was improved by that of Baker, its imperfections leſſened, and new important matters added.

I proceed to recite my experiments and obſervations, beginning with a fact which may be patent to every one. I examined the ſand from a ſewer, about three hours after it had been put into water. I had no trouble in finding what I ſought for. In the firſt drop preſented to the microſcope, appearing like muddy water and ſand, I ſaw three living beings, which I immediately recogniſed as three of Lewenhoek's wheel-animals. Upon the anterior part of the body was a horn: the ſize of the body increaſed towards the middle, and the poſterior part was terminated by three points: but the anterior part had neither trunks nor wheels, and the animals were nearly as in fig. 2. A.

The body is tranſverſely annulated, and longitudinally radiated with ſome parallel promi-

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nent rays; fig. 3. The indiſtinctneſs of the rings and lines renders them difficult to find. To be able to ſee them, one muſt be accuſtomed to obſervation, and have an excellent eye. In the middle, there is obſcurely ſeen a little longitudinal faſcia, covered with ſpecks; and above it, more viſible, a circle formed, as if by two C's touching at the extremities. At the upper part of the circle is ſeen the origin of a little canal, A B, fig. 3.

The animal being very flexile, it aſſumes ſome extraordinary ſhapes in its progreſſion. Sometimes it extends, and becomes very ſlender; at other times it contracts, and becomes very corpulent. Sometimes the anterior part is contracted and concealed in the body, or the ſame happens to the poſterior part. One part of the body will be inflated, while the reſt is flaccid. In a word, its motions are as ſingular and eaſy to ſee, as they are difficult to deſcribe with preciſion: and all thoſe remarkable figures are exhibited, although the animal remains ſtationary.

The mode in which the wheeler tranſports itſelf from one place to another, is this. It fixes the tail to the plane which it means to traverſe: the whole anterior part of the body is extended. When in this ſtate, the animal detaches the tail; and, contracting the poſterior part of the anterior, it advances. The extremity of the

the tail is again fixed to the plane; the body is extended as before; the extremity is detached; and, by the contraction of the anterior part, a new step is made. This operation is repeated, and the animalcule passes along with such agility, as soon to traverse the field of the microscope.

This method of progression, by means of the contractions and extensions of the body, is common to several insects; but, in particular, to apodal vermes, which is generally known. A circumstance peculiar to the wheel-animal is, that it fixes itself by the point of the tail; which is so essential to its regular progress, especially over a polished surface, that without this precaution, it would constantly go wrong. When the animal has found some point of support, and is fixed by the tail, it frequently stops for some time, and extends the anterior part, as if from a centre examining around, what direction it should take: then, suddenly detaching itself, it advances in a given line.

In Lewenhoek's opinion, the wheel-animal fixed itself by all the three points at the extremity of the tail. At first, I likewise thought them necessary; but, upon examining them with more attention, I perceived that the middle point only was used for that purpose. To see it with precision, the drop where the wheel-animal is, must be thin and transparent, and

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free from ſand: then it is eaſily perceived, that, ſo far from the lateral points fixing to the plane of poſition, they do not even touch it, but are diſtant a conſiderable degree; and that the point in the middle is the only fixing one. When viewed with a powerful magnifier, this point ſeems to be compoſed of a number of infinitely fine ſimilar points, which are almoſt imperceptible; Fig. 3. D. It is by means of theſe points that the wheel-animal advances.

The three wheel-animals, which I then obſerved for the firſt time, were not ſwimming; they crawled along at the bottom of the drop. I ſoon perceived, that this was their cuſtom when the wheels were not in action. Any one may ſatisfy himſelf of it, by putting a quantity of ſand, mixed with wheelers, into a watch-glaſs half full of water. He will immediately ſee, that thoſe at the ſurface of the ſand conſtantly crawl upon it, and do not commit themſelves to the water. The ſame is the caſe with thoſe buried in the ſand, when brought to the ſurface.

The three wheel-animals moved with agility in the drop which I firſt obſerved; the anterior part ſearching among the ſand as if for food; but they never left the fluid: they approached the edges of the drop, and inſtantly returned. As the drop begun to evaporate, their motion became languid; and the languor

encreased so much, as to deprive them of power to change their place. Although they remained upon the same spot, they turned around and stretched themselves. Such motions were particularly conspicuous in the head and tail, which proceeded from, and re-entered the body; and they were hid when the whole drop had evaporated. The appearance of the wheel-animals then changed, not only because life was gone, and they were completely motionless, but also because their size was greatly diminished. They became three minute corpuscula, so distorted, that it was impossible to recognise them for what they had originally been.

They were an hour in this state of apparent death. I then put upon them a drop of the same water that had evaporated. The reader may well conceive my attention in observing this resurrection, the success of which happened as I had foreseen. In a few minutes, the animals begun to swell, a point appearing at one end, Fig. 4. D. This pointed part then begun to move, by extending and contracting: the part opposite became pointed also, and begun to move as the other. I perceived that those points were the head and tail of the animalcule, proceeding from the place where they had been concealed upon the evaporation of the drop. The transverse annuli, the longitudinal rays, the internal and external organs, all

all re-appeared; and the three wheel-animals resumed their original figure and size, which they in a very short time effected. They traversed the sand vigorously, and shewed themselves to be as lively as ever.

Discovering some wheel-animals in the sand of sewers, I repeated those experiments, and found that they always revived, during whatever time they had been kept dry. There is now before me a remarkable instance. I have still in my possession some sand, upon which I made experiments near four years ago; which has been preserved dry in a small glass vessel. When moistened, the wheel-animals in it instantly revive. This fact agrees with what I have related from Lewenhoek. Baker has observed a circumstance little less worthy of notice. With water he wet the inside of a glass wherein wheel-animals had been kept dry for some months, and he saw them recover their original vivacity. Here it matters little to know whether they revive oftener than once. Eleven times I have dried the same sand, and as often wet it; and I have constantly seen the death of the wheel-animals attend the drying of the water, but their life recommence when the sand was moistened.

However, those facts should be understood under some limitations. Although the animals do

do revive several times, and even after they remain a long time dry, it is certain that the number revived always decreases in proportion to the times the sand continues longer dry, and the times it has been wet, to revive the wheel-animals. It is true, I have seen their eleventh resurrection. The first times, they were very numerous; but the number afterwards continually decreased, and at last became very small. I should add, that, still wetting and drying the sand, none revived the sixteenth time. It is the same with respect to the sand remaining dry. I took a portion numerously inhabited by the animals, and I have preserved it dry in a box for three years, only moistening it every five or six months for my observations; but the resurrections always become fewer; and now, at the end of the third year, I do not exaggerate when I say, that one hundredth part does not revive. I have not extended the experiment further: but it is undoubted, that if the wheel-animals remain long dry, and if they continue to become rare in proportion, a period will finally arrive, when they will revive no more.

The time necessary to accomplish their resurrection, is unlimited. I have found, that in four minutes after moistening the sand, some begin to live; life then extends to more; and in

in an hour, all are reanimated [a]. I am ignorant what can occasion this difference in the time necessary for their resurrection. It may happen, either because some parts of the sand where some of the animals are, may be better moistened than others; or, although all should be moistened in the same time, it may happen, that all the wheel-animals are not of the same texture. In that case, the more dense or consistent will more slowly receive the impression of the water, and be longer in reviving; or, some may be diseased, and less fit for immediate resurrection.

I have not perceived any very sensible difference of time between the resurrection of those which have been dry for some hours, and the resurrection of those which have been dry for several days, several months, or even for complete years.

As I knew the influence of heat in restoring the life of animals and vegetables, I frequently moistened the sand with warm water. The wheel-animals then revived sooner than when the sand was moistened with water at the heat of the atmosphere.

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[a] Baker's wheel-animals begun to exhibit signs of life only in half an hour. It would appear, that he speaks of those that were longest of reviving. Perhaps his experiments were made upon a species different from mine.

But there is one condition essential to effect the resurrection of the animals. It is absolutely necessary, that there should be a certain quantity of sand. Let us enquire further into this. One day, I had two wheel-animals traversing a drop of water about to evaporate; it contained very little sand. Three quarters of an hour after the evaporation of the drop, they were dry and motionless. I moistened them with water, that they might revive; but this was in vain, although they continued wet during several hours. Their members swelled, and they became three times their original size: but they still remained motionless. This circumstance seemed to me the more extraordinary, as it was among the first times I had wet the sand, and those two were the only animals that did not revive. To be certain whether the fact was merely accidental, I took a portion of the sand, and spread it out upon a piece of glass: I waited until the numerous reanimated wheel-animals should become dry, in order to wet the sand anew, and learn whether they would revive or not. The sand was carelessly scattered upon the glass, so as in some parts to be a thin covering, and in others to be in a very small quantity. The animals in these parts did not revive; but all that were in those parts where there was abundance of sand, revived. A difference so remarkable,

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made me ſuſpect that this ſpecies of beings inhabiting ſand, require a certain portion of it to enable them to paſs from death to life.

However, to acknowledge the truth, I did not at firſt adopt this conjecture; but I could not diveſt my mind of it, as it ſeemed to be confirmed by facts: beſides, upon recollecting the experiments I had before made upon the reſurrection of wheel-animals, I was certain, that thoſe reviving always had been among ſand. To certify or deſtroy the doubt, it was ſufficient to repeat the laſt experiment; for, if thoſe which revived had been mixed with ſand at the moment of their reſurrection, or if, upon the contrary, thoſe then without ſand did not revive, there would be a complete demonſtration of what had been the cauſe; namely, that the preſence of ſand was eſſential to their reſurrection. Upon repetition of the experiment, the conſequence conſtantly was, that the animalcula never recovered life, unleſs in places where there was a quantity of ſand.

One of my friends, an eminent philoſopher, and an excellent microſcopical obſerver, conſtantly has the ſame reſult from his experiments *.

The Abbé Roffredi, a good obſerver, when incidentally ſpeaking of the wheel-animal, in the

* Il. R. Padre D. Carlo Giuſeppe Campi di Milan.

the Abbé Rosier's *Journal de Physique*, mentions the same phænomenon [b].

To these we may add the following facts. If we spread out the sand with wheel-animals, in such a manner, that there is a considerable quantity in some places, and much less in others, and very little in the rest; and then moisten the whole: in the first case, where it is plentiful, the reviving animals will be numerous; in the second, where it is more rare, fewer will revive; and in the last, very few, sometimes none. If the most abundant quantity of the sand is dried and scattered thinly over any place, then there will appear only some animals revived, where they were before very numerous. Besides, the resurrection of the wheel-animals in this small quantity of sand is effected later than where the sand abounds: in this case, four minutes is enough: where there is little sand, their resurrection will require nine, or eleven minutes, and sometimes more.

But, have the animals which do not revive from a defect of sand, and resemble globules floating upon the water: have they lost the faculty of resurrection, or do they resume it upon being supplied with their native sand? To ascertain this circumstance, I have often taken the thin surfaces of sand, wherein the wheel-animals

b Journal de Physique, 1776.

animals did not revive, and put them at the bottom of a watch-glaſs, with water upon them; but of twenty dead animals I ſaw ſcarcely one revive. It therefore appeared to me, that privation of ſand deprived the wheel-animals of their innate faculty of reſurrection.

How can the ſimple defect of ſand produce ſo important an effect? What connexion, what phyſical relation is there between the preſence of ſand, and the reſurrection of the wheel-animals? May not the cauſe which effects this phænomenon be entirely different; and cannot we ſay, that the ſand may ſerve the place of ſome very ſimple external condition? I ſee that when the animals periſh from want of ſand, their bodies are expoſed to the influence of the air upon the evaporation of the water; but they do not experience this, or at leaſt in a far leſs degree, when they die covered with ſand. We may ſay, this ſuppoſes that the immediate action of the air, by irritating and injuring the corpuſcula, from its lacerating influence, while they are ſtill humid, and while they are at the ſame time moſt tender and delicate, renders them incapable of reviving, from the alteration they undergo. The conjecture hazarded is founded on a fact, evincing there are animals, whoſe ſtructure is ſo delicate and fragile, that they are unable to ſupport the immediate impreſſion of the air, and

and always live under cover. Such are the *miners*, a ſpecies of inſects ſo named from their inhabiting the interior of the leaves of trees, where they live almoſt always concealed and protected from the influence of the air. My conjecture would perhaps require an experiment, which I had not time to make. We ſhall immediately ſee, that wheel-animals revive in vacuo. One might put ſome of thoſe, ſwimming in water without ſand, under an exhauſted receiver, and then wet them, obſerving whether they revived; which they ſhould, according to my ſuppoſition; becauſe, in that ſituation, they could ſuffer nothing from the motion of the air when the water evaporated.

I arrive at another enquiry, more important than the preceding. I have hitherto ſuppoſed, that the wheel-animals *periſhed* when the liquid dried. It is true, they exhibit every appearance of death; the body is dry and disfigured; they loſe the uſe and motion of their members. But I mean to examine this circumſtance more deeply, ſince it preſents the moſt paradoxical truth hitherto found in the hiſtory of any animal, and we cannot heſitate too much in diſtruſting truths of a ſimilar nature. Let us therefore enquire, if it is not poſſible that the wheel-animals, dead to appearance, may preſerve a ſpark of life. To elucidate our enquiry, let us recur to the analogies between

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large animals. Cold, ſo injurious to inſects, renders thoſe it does not deſtroy, lethargic during winter: they are in ſuch a degree of torpor as to ſeem dead to the ſight and the touch; their limbs ſtiff and contracted, their wings depreſſed, their bodies emaciated. They have no external motion; no mark of feeling when ſtimulated, or when cut in pieces. This we ſee in the hundreds of inſects which we caſually find upon the coldeſt days of winter, in the earth, in the clefts of trees, in the holes of walls. In this manner does cold operate upon beings poſſeſſing the higheſt rank in the animal ſcale. In the midſt of winter we have found marmots, which are a ſpecies of rat, ſo lethargic, that the flame of a candle burning their limbs could not awaken them, or recal the ſenſations of life[a]. Terreſtrial and amphibious animals, when kept long in water, exhibit the ſame appearances. Rhedi having immerſed flies in water an hour and a half, found them with all the ſemblance of death. Reaumur made the ſame experiment upon bees. It is well known they are vindictive when injured. This naturaliſt left a whole ſwarm in water, I know not how long, and found them ſo completely deprived of ſenſation, that he handled them at pleaſure; he took them from the water, put them upon a table,

[a] Buffon, hiſtoire naturelle.

table, and examined whether or not there were ſeveral queens: they afterwards recovered. I may ſay the ſame of the apparent death of frogs and newts. After ſome hours immerſion in water, their bodies became ſtiff and fragile, juſt as happens in death.

May not the apparent death of inſects and other animals be ſimilar to that of wheel-animals among ſand? But thoſe animals preſerve a real principle of ſenſation and life, which the concurrence of certain circumſtances is required to unfold, and put in a ſtate to animate the whole ſyſtem. If the temperature of the air is a little encreaſed, motion and life reappear in animals rendered torpid by cold: if the bees which have been immerged in water are expoſed in the ſunſhine, they ſoon begin to move, walk, expand their wings, and fly. In the ſame way do frogs and newts recover their natural vivacity, when they have remained a ſhort time in a dry ſituation. Why then cannot we ſay the ſame, that there is in the wheel-animals ſome latent ſpark of life, which the aid of water is required to diſcover?

Conſidering theſe facts, and allowing them their juſt value, I ſee that we cannot deny that there is a reſemblance between the ſtate of dry wheel-animals, and the ſtate of the animals I have named, with reſpect to their appearance, their perfect immobility, and the complete in-

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action of their members. But I remark a most sensible difference, which must create a great distinction between them. When animals are torpid from cold; whatever is the power of the agent depriving them of sensation and motion, it only does this, by destroying the harmony between the fluids and the solids; but it does not derange them so far, as to deprive them of what constitutes their fluidity or solidity. This harmony exists in the most internal parts of the body. I have often opened newts, frogs, toads and lizards, when torpid from cold, and apparently dead; and I have found that the blood did not circulate in the limbs, while it continued to circulate in the large vessels, although the circulation was languid. If a greater degree of cold has penetrated the solids, if it has coagulated the blood, then, it is certain, the animals perish. This has already been observed in many animals, by several naturalists; and I have myself found the same in the toads, frogs and newts, of which I speak.

I have found a remnant of motion in the blood and in the heart of half-drowned animals; and I doubt not that this motion continues in bees and flies. When all those animals remain long in water, the motion, whatever it is, is destroyed, and all hope of recovery is lost. It is therefore very certain, that, in the animals which revive, that quality which constitutes

constitutes the existence of their solids and fluids is not lost, and the harmony which reigns between them is not totally destroyed. But, how many essential differences are there in wheel-animals? While they traverse the fluid, they resemble gelatinous substances; they are lacerated and destroyed when touched with the point of a needle. When dry, the solids are contracted and distorted; the whole body of the animal is reduced to a hard shapeless atom of matter: when pierced by a needle, it breaks in pieces like a grain of salt. But how does this atom of matter, where the solids preserve no vestige of their former humidity and pliancy, and where the fluids exist no more; how can we believe that this dry and disfigured atom retains a principle of life? Can we think that life exists in a frog, a toad, or a newt, when dry and rigid, as the wheel-animals are among the sand? Can we conclude, and conclude with reason, that in dry and shrivelled wheel-animals, we must admit the same as in the reviving animals I mentioned before? Can we conclude, I say, that their life is entirely gone, not only because the reciprocal action of the fluids and the solids is destroyed, but also because the fluids are entirely evaporated, and because the dryness and rigidity has changed the natural state of the solids? If, when we put a stiff and contracted frog,

frog, toad, or newt, in water, we faw it gradually become animated; then, as we would fay, it was a real and abfolute refurrection: fo may we call what happens to the reviving wheel-animals, a real and abfolute refurrection alfo.

But it is time to refume the hiftory of thofe wonderful animalcula. We have already defcribed their figure and their properties; but we have not yet examined their organs feparately, which is neceffary to make us well acquainted with their ftructure and motions. I mean to fpeak of the heart, the two trunks, and the wheels revolving at the vertices. I could not properly treat of them before, becaufe it would have prevented me from purfuing the plan I intended to adopt, which led me to relate fome facts, following the order of the time when I learned them. In all my obfervations, the wheel-animal has fhewn me thofe three organs only. Their appearance depends entirely upon the will of the animal. While animated, it is not unufual that it does not fhew them at firft, or difplays them very flowly. This is what I have obferved in my wheel-animals, and what fome of my friends have obferved along with me. I did not obferve all the three organs until after twenty-one days examination. Let the reader reprefent to himfelf a fnail proceeding from its fhell; it extends

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itself, and puts out its head and horns; then retiring within its habitation, contracts itself, and conceals the head and horns in its body: in this way, he will sensibly figure the motions of the trunk and the wheels of our animalcula. The wheel-animals I then examined, and those which I afterwards saw, did not always at the same moment display both the trunks and the wheels, but, like snails, concealed sometimes the one, and sometimes the other; which happened whenever they contracted themselves; and when they remained long extended, the trunks and wheels were kept out a long time also. The wheels cannot receive this appellation but in a very improper sense, and by means of a sort of latitude or accommodation. In the tract upon the Animalcula of Infusions, I have treated at length of the minute, long, and slender fibrils proceeding from the orifice of the mouth of many of those beings: I have said, the fibrils were in constant vibration; that they produced a certain vortex in the infusions, which drew to the mouth of the animalcule the corpuscula serving it for food. The wheels observed in the wheel-animals, are only two circular lines of those fibrillæ constantly in motion: they produce the same effect with the vibrating points or fibres of infusion animalcula; they form in the water two great vortices, which convey to the animal the substances

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whereon it feeds. A wheel-animal is exhibited with the trunks extended and the fibrils, whose motion resembles that of two wheels, and forms two vortices; Fig. 5. pl. 3.

When I say that the wheels of my animalcula are such only in appearance, I do not mean to insinuate that this will extend as a general rule to all. This optical illusion, indeed, has been corrected by some naturalists, in particular by Messrs Trembley and Bonnet; but it is also certain, that the opinion of celebrated observers is different. Lewenhoek, that acute observer of the most minute objects, actually calls them wheels, which revolve like those constructed by mechanics. Baker, who is not inferior to him in accuracy of observation, and who has studied the wheels attentively, to discover whether they are truly such, or only vibrating fibrillæ, is very much inclined to believe them wheels. The observations of these two illustrious observers may very well agree with mine; for their wheel-animals have been different. A singular aperture for a mouth, situated between the wheels, a sort of ring beneath it, a number of serpentine vessels in the head, the peristaltic motion of the intestines, the irregular agitation of a transparent fluid in every part of the body, a particular undulation of that fluid in the intestines and the skin, were observed by the English philosopher in his wheel-

animals; and although he has accurately described them, I have been able to find none similar in mine. There is no doubt, I might have seen all those organs, both because I used Cuff's microscope, as Baker had done, and also some microscopes which were much superior. With this different organization, it is not wonderful that his wheel-animals exhibited another organ which I have not found in mine; that is, a pair of wheels proceeding from the two trunks, the revolution of which produces the same effect as the vibration of the fibrillæ: a rapid current is formed in the fluid, which carries the food to the mouth of the animal. With Baker we must remark, that this apparent rotation is not always made with the same velocity, and in the same direction. Sometimes it is quicker and sometimes slower, which alternatives are executed momentaneously or by degrees. In the same manner, we see the animal at one time turn to the right, and immediately afterwards to the left. It frequently happens, that after it has moved long at one side, it stops and turns diametrically opposite.

Let us leave this digression, and return to the animals. When their fibrillæ appear, they no longer crawl at the bottom of the water, but they swim through the whole fluid with the greatest velocity. Examining them while they swim, I have often endeavoured to discover, whether

whether they ſwim by the undulations of the body, or by the vibrations of the fibrillæ, which not only regulate the vortices, but, by their action againſt the water, may raiſe the animal. This I have not been able ſatisfactorily to elucidate; but I ſhould think they ſwim by the vibrations of the fibrillæ, ſince they ſink when theſe are drawn within the body.

I have already ſpoken of a little circle ſituated towards the head of the wheel-animals, which is in appearance like the junction of two C's by the extremities. This part is in conſtant motion, by alternate contraction and dilatation, while the animal forms the vortex, and while the fibrils are extended. The part has been obſerved by Lewenhoek and Baker: they have thought it the animal's heart. The ſituation, the ſhape, contracting and dilating motion, concur, according to the Engliſh naturaliſt, in eſtabliſhing this opinion. But, if this is a heart, it is a voluntary muſcle, which beats at the pleaſure of the animal; that is, when it protrudes the fibrillæ and forms the vortex; and this voluntary act has, before me, been obſerved by others. But, are there animals whoſe hearts beat by intervals, ſo that the beating may ceaſe when the animals chuſe? Beſides, the wheel-animals ſometimes remain alive in the water for ſeveral weeks, without making the vortex, conſequently without moving the

 heart.

heart. Is it poſſible that any animal can live ſo long without the beating of the heart, the animating ſpring of the whole machine? Theſe are two paradoxes, which may be no leſs true than there are others more wonderful: ſuch as, the reſurrection of the wheel-animal itſelf. Although it may be thought that this part is actually the heart, either from performing ſimilar functions, from its ſituation in the region of the breaſt, from its contraction and dilatation, like a heart; I cannot think theſe are convincing reaſons; for it may be an organ deſtined for a purpoſe completely different. I ought to ſay, as I think, that it is more natural to believe this organ ſerves for the aliments; ſo that it contracts and dilates to receive the nutriment, and tranſmit it to the ſtomach. Such an hypotheſis will eaſily explain why it is in motion only while the vortex is formed: it is becauſe the aliment is then drawn to the mouth and tranſmitted to the body. If the part remains long motionleſs, it is becauſe no food is taken, and this commonly happens while the animals, in an unſuitable ſituation, languiſh and die; which is ſometimes the caſe with thoſe revived in ſand kept in cloſe veſſels. I have ſeen, that although the ſand ſwarmed with wheel-animals during the firſt days, the number decreaſed; and the decreaſe was to ſuch a degree, that in twelve or fifteen days, the whole

were

were dead. They appeared motionleſs and disfigured at the bottom of the veſſel, and a great many of them were even reduced to nothing. During this period of diſeaſe, the vortex was ſeldom made; but it is moſt frequently formed by them when we find them in ſewers, and in pits full of rain water.

The idea, that this moving particle is an organ formed for the reception of the aliments, in order to tranſmit them to the ſtomach, is not imaginary. It is founded, 1. Upon obſerving in my wheel-animals a kind of little canal united to this part, Fig. 3. B, Fig. 4. E. The canal riſing towards the head, has great reſemblance to an œſophagus. 2. This part is ſurely deſtined for that purpoſe in other aquatic animals, bearing a great relation to the wheel-animals, and afford cogent evidence of the fact. Such is an animal often found in the tremella. It is ſhorter, and a little thicker, than the wheel-animal. The poſterior part is provided with two ſmall diverging filaments, with which it fixes itſelf to any ſubſtance. At the anterior part, there are long ſlender fibrils, which being put in motion by the animal, occaſion a vortex in the water, Fig. 6. There is no vortex when the fibrils are at reſt: while they move, and during the continuance of the vortex, there is ſeen, almoſt in the centre of the animal, a particle, A, ſimilar in figure to

that part in the wheel-animal of which we ſpeak: it alternately contracts and dilates; but the motion ceaſes with the ceſſation of the vortex. This difference only is to be remarked; the particle in the wheel-animal is formed by two ſemicircular cavities; whereas that in the tremella animal, reſembles a bladder or folliculus. The particle and canal towards the region of the head, are connected as in the wheel-animal. There is a ſhort duct, B, terminating at the mouth of the animal, and at the oppoſite extremity; it enters another folliculus, C, which moves with alternate contraction and dilatation, undulating like a wave almoſt at reſt. This folliculus is preciſely the receptacle of the aliments. It is always full of a yellowiſh green matter, which proceeds from the poſterior part of the body, by means of the undulating or periſtaltic motion. But we not only ſee the food diſcharged from, but we alſo ſee it enter the body; that is, we obſerve fragments of the tremella drawn by the vortex to the animal's mouth, infuſion animalcula of various ſizes, and fragments of other ſubſtances. Some of the moſt minute enter the origin of the œſophagus, traverſe the moving particle, and arrive at the paſſage we ſee in the receptacle of the aliments.

The ſame thing is obſerved in another animal of the tremella, which I have mentioned

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in the tract upon the *Animalcula of Infusions*. The moving particle of this animalcule, for it has one as well as the wheel-animal, contracts and dilates, while the aliment collected by the vortex passes from the œsophagus to the stomach.

In those animals, therefore, we see this particle, which, in place, figure and motion, resembles an heart, although it is not one, but is an organ destined for the use of the aliments. For these reasons, I have ascribed the same use to that of the wheel-animal.

Should I be right the wheel-animal has no heart, we see no other part, no other organ, that can merit this name. If we may judge of this by the senses, I say it has none, more than the two tremella animals I have mentioned, a number of infusion animalcula, the prodigious abundance and variety of polypi visible by the microscope and to the naked eye. I stop at them, to say no more of many other animals. I have never seen the appearance of circulation in wheel-animals, nor in the animalcula of infusions, those of the tremella, nor in polypi. Although Baker has observed in wheel-animals, the irregular agitation of a fluid, he ingenuously avows, that he never has perceived any trace of a real circulation: yet all feed, encrease and multiply, as those animals which have a heart and circulation. Neither are these es-

ſential to the life of many animals. For this, it is enough that there is a juſt equilibrium, a correſponding harmony, between the fluids and the ſolids. The ideas we have of a heart and of circulation, are particular notions, taken from a definite number of animals, which demonſtrate the bounds of our knowledge and intelligence, and which would ill apply, if we meant to fit them to the immenſity of models framed by nature.

The wheel-animals inhabiting the roofs of houſes, of ovens, or other buildings expoſed to the inclemencies of the weather, ſhould be of a conſtitution calculated to ſupport the moſt ſevere influence of cold and heat. I put them to the teſt. From a ſewer expoſed to the ſouth, I took wheel-animals ſand, which had for twenty-nine days been ſubjected to the heat of the ſun in the middle of Summer. The thermometer in the ſunſhine ſtood, during this time, at 129, 131, 133°. The heat did not injure the animals; for, when the ſand was wet, I had a great number very vivacious.

I expoſed a little of this ſand in very thin glaſs tubes, without a ſouth window, where the reflection of a neighbouring wall excited an extreme heat, and I left them there the whole Summer. During ſome of the hotteſt days, the thermometer roſe to 142°; but the heat did not injure the wheel-animals: upon wet-

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ting the ſand, they appeared with the ſame livelineſs and vigour, and in the ſame abundance, as in other ſand from the ſame place expoſed in a north window, and ſeldom or never experiencing the ſolar rays. I have concluded, that the powerful heat of Summer did not deprive the wheel-animals of the faculty of reſurrection; but, is it the ſame when they are revived? Is this degree of heat then equally ſupportable?

I have alſo expoſed thoſe tubes with ſand and water, where there was a number of wheel-animals, in the warm ſituation above mentioned. The effect was very different: in half an hour, the heat of the ſun at 135° killed the animals. Thus, it is not the ſame with wheel-animals, when dry and deprived of life, as when in life and motion.

I afterwards ſaw, that reſuſcitated wheel-animals died when expoſed to a more gentle degree of heat, when expoſed in the ſunſhine at 113°.

The heat of the fire has the ſame effect as that of the ſolar rays. But, while the revived animals periſh at 111 and 113°; if expoſed to 144° when dry, they do not loſe the faculty of reſurrection. With common fire, I could extend my experiments farther than with the heat of the ſun. I raiſed the heat to above 144°, to ſee whether dried wheel-animals ceaſed to revive; for it was to be thought there were

limits

limits here; and I found thoſe limits at 153°. Sand expoſed to this degree of heat, preſented few wheel-animals, and at 158° none. But there is a circumſtance here neceſſary to be elucidated. I made thoſe experiments, as I may ſay, dry, keeping the ſand for two or three minutes expoſed to the degree of heat I fixed upon. The conſequences were very different, when I uſed wet ſand, and immerſed it two or three minutes in water warmed to that degree; then, the wheel-animals did not revive beyond 131°.

It is not difficult to explain, why the deſtruction of re-animated wheel-animals is more eaſy than when in their ſtate of deſiccation. The former are a ſort of jelly; of conſequence, very fragile. Their minute filaments are eaſily broken and deſtroyed by the penetrating power of the fire, which does not take place with ſuch facility when they are dry: then they are hard, and partly concentrated in themſelves. Beſides, in this ſtate, their globular figure preſents leſs ſurface to the action of the fire; alſo the heat acts alone upon dry wheel-animals; and, when they are alive, it acts along with the water, which powerfully concurs in lacerating them, and deſtroying their organization: and the heat has rendered that lacerating power more active and penetrating. It is for this reaſon that

that dry wheel-animals can leſs reſiſt the heat of warm water, than of the fire itſelf.

Having ſeen the effect of heat upon wheel-animals, it was neceſſary to ſee the effect of cold upon them. For this purpoſe, I took the ſand of ſewers, of bent and flat tiles, where the animals are found during the moſt intenſe cold of winter, when the roofs are covered with ſnow and ice. The ſand moiſtened with water, by the cold became ſo firm and connected, that it was as hard as a ſtone; but this cold had not injured the wheel-animals. After melting the mixture of ice and ſand, I ſaw a great number ſoon revive, only their reſurrection ſeemed to me leſs immediate.

The greateſt cold in Winter was 16°: I thought therefore of ſubjecting thoſe wheelers I found upon the roof, to a degree more intenſe; and, taking ſome portions of frozen ſand from the bottom of a ſewer, I put them in a little glaſs veſſel, which, for three hours, I placed in a degree of cold 11° below 0, by means of a mixture I have often mentioned: But the animals revived when the ice melted, which prove, that degree of cold had done them no injury.

After being certain of this, I ſought for the reſults of the experiment inverſed; that is, what will happen when we paſs wheelers from the degree of heat at which they are animated,

mated, to different degrees of cold, always more intenſe. One morning I tranſported ſome ſwimming in a watch-glaſs to a north window, where the thermometer ſtood at 27°, obſerving what happened when the water in the glaſs became ſo cold that the hand could ſcarcely be kept in it. The wheel-animals ſtopped the vortex, and fell to the bottom, crawling languidly along the ſand. The water was ſoon frozen; then they moved with difficulty; and this motion in a ſhort time ended. When the water was more frozen, they contracted within themſelves, forming into globules, which I clearly ſaw from the tranſparence of the ice. Thus, the wheel-animals paſſed the whole day and the following night, which was very cold. The next day I removed them to a warm apartment, to ſee whether thoſe in the ice, under the figure of globules, would recover life and motion when the ice melted. This took place even when I made the globules remain longer in ice: even when I encreaſed the natural, by means of factitious cold, to 11° under 0.

Reaſoning from the experiments of cold, as upon thoſe of heat, it would appear, that the revived wheel-animals ſhould not ſupport the ſame degree of cold as thoſe which are dead. If, upon the other hand, theſe laſt facts do ſhew, that when the cold begins to act with

with power upon revived wheel-animals, they then paſs from life to death, as appears by the ceſſation of their motion, their contraction and disfiguration, ſo that they become preciſely as when the evaporation of the water leaves them dry in the ſand.

Beſides, ſuppoſing that an extreme cold exercises its influence upon living wheel-animals, I know not if it could have the power of depriving them of life. It is certain it could not deſtroy animalcula, as delicate as ſome of thoſe of infuſions, and the eels of vinegar. But, what is more ſurpriſing, the animalcula of infuſions, and the eels of vinegar, periſh at a leſs degree of heat than the wheel-animals. They cannot ſupport more than 111° above 0. At this degree of heat, ſeveral ſpecies of beetles, of chryſalids and caterpillars, periſh, as I before obſerved, although they ſupport the cold of 34° under freezing. By this we ſee, that ſeveral animals ſupport cold better than heat.

Thoſe facts teach us, that there are two principal cauſes deſtructive of the reſuſcitating quality in wheel-animals. Theſe are, the want of ſand and of heat. But, are there alſo others producing this effect? I could not diſcover this but by conjectures, and by uſing the different methods which are noxious to the production and life of other animals, eſpecially thoſe that have the greateſt analogy with wheel-animals; ſuch as the animalcula of infuſions. It has

been

been proved, that these are produced in vacuo. This method of injury does not seem sufficient to prevent the resurrection of wheel-animals, though we cannot deny that their resurrection is facilitated by the influence of the air. The mean of the principal results of repeated experiment is, 1. Wheel-animals revive sooner, and in greater numbers, in the open air, than in vacuo. 2. Those which do not revive in vacuo, revive when put in the open air.

However much the air may promote the resurrection of wheel-animals, it is absolutely necessary for the preservation of their lives. When they revive in vacuo, or are put in an exhausted receiver, they die in a few days.

Although wheel-animals revive in vacuo, it is not so successfully as in the open air: it is natural to think they would revive in confined air, although this air prevents the production of other animals, and kills them when confined in very small vessels. But in the vessels where I had the wet sand of those animalcula, although hermetically sealed in small tubes, they have always very soon revived, and in abundance: they have even lived long in very small vessels, where there was very little air.

Wheel-animals suffer from different fluids, what they do not suffer from privation of air, or from confined air. I shall mention the liquids that do, and do not, injure them. Those which

which are not noxious to them, are either thofe in which they revive, or in which, after refurrection, they preferve life. Such is, pit, river, ice, fnow, and rain water; diftilled water, that of ditches, marfhes, pools; the fœtid water of mud and dunghills. Refpecting the fluids noxious to them, they are either thofe impregnated with pepper, common falt, falgem, vitriol; thofe in which are expreffed the juice of the onion, garlic, urine, ink, wine, verjuice, oil of olives, of nuts, brandy, vinegar, &c. Having put in each of thofe fluids, the fand of wheel-animals, I never faw one revive; likewife, when revived wheel-animals were put into them, they all perifhed. Some ftrong and penetrating odours have been equally fatal to them. Such is the odour of camphire. When revived, if they are long fubjected to it, all die, and do not revive when dry. The oil of turpentine produces only the firft effect. But if this odour becomes more active, as by melting or burning the turpentine, the fumes prevent the revivifcence of the animals. The fumes of burning fulphur and camphire produce both effects: the fumes of leaf tobacco only deftroy revived wheel-animals.

Reflecting upon the experiments made by means of heat, liquids and odours, I have fometimes doubted whether thefe three agents had

had deprived the wheel-animals for ever of the property of reſurrection, or if there was any place to hope it might be recovered. This hope did not ſeem chimerical in a being like the wheel-animal; it would not be wonderful to ſee it recover the faculty it naturally poſſeſſes. I have preſerved ſand which has been expoſed to heat; I have from time to time wet it with pure water, and have often obſerved it. The ſame has been done with ſand expoſed to liquids and odours, keeping it in the air and wetting it with freſh water, that the noxious qualities which injured the wheel-animals might be deſtroyed. But theſe methods have never re-animated the numerous dead bodies.

The wheel-animals which ſuggeſted the opinions I have laid down, were for the greater part thoſe found in ſewers, the ridges of flat and crooked tiles. In this matter, which for brevity I have called ſand, though, to ſpeak more properly, it is a mixture of earth, ſand, and the fragments of tiles, (I ſhall continue to call it ſand), is the dwelling of wheel-animals: but we muſt obſerve, that in ſome kinds they are much more numerous than in others. If the ſand is red, it is almoſt a certain indication, according to Baker, of the preſence of thoſe animals: but they are always inanimate when the ſand is dry. By one who is accuſtomed

customed to observe wheel-animals in the state in which they are found, when by the evaporation of the water they are dry, they are easily recognised when the sand is presented upon a slider to the microscope: then they are seen in the shape of minute dry globules, of a reddish yellow colour; which, when moistened, extend themselves to form the animals.

The animals are also found in certain waters of the earth. Both Baker and I have often seen them in ditches; and I have taken many from pools, marshes, and even holes of water.

The wheel-animals of the earth are, in my opinion, the origin of those of roofs; and it seems to me that it cannot be otherwise, at least, that we cannot say the wheel-animals of one roof come from another, which supposes a particular case: but to take the case generally, and consider it as in its origin, I should derive them elsewhere, and consequently recur to the waters.

The manner in which they pass from the earth to roofs, may be easily conceived; when in their dry state the wind may effect this, or when the air has raised the whole or part of the water where they are.

While studying those curious beings, I have always reflected upon a most important problem; which was, to enquire into the mode

of their propagation. For this, I have isolated them in watch-glasses like infusion animalcula: I have put one in each, but I never could see them propagate, either by shoots or divisions, although both ways are very common among aquatic animals. Neither was it by a fœtus; but I had reason to think, that it was by means of eggs. Whenever the isolated wheel-animals had been some days revived, I have seen, in the body of the largest, an ovular substance; Pl. 3. fig. 5. N. When I happened to find the wheel-animals dead, they had always this ovular body; but in general it had passed from their own bodies into the glass, without my knowing how; at the same time, with an important singularity. When entire, the isolated animal swam alone in the fluid; but when the body was broken, there swam another wheel-animal, much smaller, along with it. This made me suspect that the new inhabitant had come from the ovular substance; especially as, when the animalcula were excluded, the other eggs were broken. It might likewise be suspected that it had been carried thither by the air: but to be sure of this, it was necessary to see the animal's departure from the ovular substance; which, notwithstanding all my care and attention, I could never accomplish.

My observations agree with those of Baker, who has not been more successful than myself. He

He thinks the wheel-animals are oviparous; becaufe he has often found, in the water along with them, gelatinous eggs of a proportional fize. Befides, he has in two fpecies of wheel-animals, a little larger than the moft common kind, difcovered an oval body, the figure of which very much refembles the fubftance I have defcribed; but he has never feen one excluded: nor has he feen a wheel-animal come from the gelatinous eggs, although he has kept them three years.

The learned Padre Roffredi has enjoyed the happinefs denied to Baker and to me. In the *Journal* of the Abbé Rozier, he has given a finifhing ftroke to our obfervations. Speaking of the wheel-animal, he incidentally fays, but in exprefs terms, that he has feen it proceed from the egg. "Lewenhoek is miftaken in "thinking the wheel-animal viviparous; and "what he takes for fhapelefs excrements in "the inteftines, is really an egg which I have "feen it produce, and obferved it many times "until I faw it hatched." If Signor Roffredi's obfervation is correct, I cannot doubt the fact. By this means, a circumftance in the hiftory of the wheel-animal is elucidated, which fhould deeply intereft the naturalift.

The folution of this problem, added to my obfervations, difcovers another important truth; namely, that the wheel-animals undergo no

metamorphosis. I have collected many minute ones produced in a watch-glass, and kept them with great care: they always continued to encrease: but when I killed and revived them, their increment was slower than when they were kept continually wet. When full grown, each in general laid an egg, which produced another wheel-animal. Thus, I know, that from the time they are produced, until maturity, they undergo no metamorphosis. We cannot say it is a metamorphosis when they are produced; for they have then attained their greatest perfection. The insects which metamorphose, never propagate their species until they become winged animals, and have acquired the last degree of perfection to which metamorphosis will bring them.

Finally, my observations have demonstrated to me, that wheel-animals are hermaphrodites in the most rigorous sense. I have had the fifth generation, from many eggs isolated in watch-glasses, making use of isolation to take away all suspicion of copulation.

CHAP.

CHAP. II.

The sand of tiles, the mud of ditches, and of marshes, which pass in the vulgar eye for the vilest of matter, are to the philosophic observer a source of wonder, from the singular beings they contain. To ditches and marshes we owe the armed, club, funnel, bulb, and knotted polypus. It is there we find the fresh water worm, the boat worm, and the springing millepede. Those animals have confounded the human mind, and have created a new philosophy. When the sand of tiles does not serve for an abode to wheel-animals, it will not for that reason be less famous or remarkable. An animal, which revives after death, and which, within certain limits, revives as often as we please, is a phaenomenon, as incredible as it seems improbable and paradoxical. It confounds the most received ideas of animality: it produces new ideas, and becomes an object no less interesting to the researches of the naturalist, than to the speculations of the metaphysician. But the celebrity of this sand will encrease, when we learn, that it contains other animals, which, like the wheel-animal,

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possess the property of resurrection; so that we may almost say, all the animals living in sand are destined to be immortal. I have discovered in sand two new species of animals, which I proceed to describe. I lament that their rareness has prevented me from extending my observations as far as I could have wished, or rather as far as the importance of the subject would have required.

Upon wetting the wheel-animals sand, I several times observed a yellowish animal, three or four times as large as a wheel-animal, with six legs; but I paid no attention to it, as I thought it was some little terrestrial insect that had fallen by chance into the watch-glass where the sand was kept. The reason inducing me to think it such, was, because I had always seen it move obliquely, and very slowly, at the bottom of the water; as if unable to walk, it was frequently supine, and always made efforts to recover its natural position; but those efforts were in general fruitless, as happens to many aerial and terrestrial insects falling casually into water. At the same time, with a more continued and careful observation, I recognised it as an animal really aquatic. I saw that it walked in so laborious and awkward a manner, from the smoothness of the glass-slider upon which it had been put for examination; and, when placed upon sand, it had a regular

progressive

progressive motion, slow indeed, and, compared with the wheel-animals' motion, resembling the crawling of a tortoise. Thus, to design it by some descriptive name, I called it the *Sloth*.

The whole body is granulated; the anterior part is obtuse; and the posterior part terminated by four hooked filaments, which serve to fix it to any particular place. The limbs have small shining claws, which, as far as one can judge, are of a corneous substance, the points turned towards the body, as we see in the recurved claws of several insects. The corpulence of the sloth rendering it opaque, prevents us from seeing the internal organization. It only allows us to see a small elliptical spot in the middle of the body, which I suspect to be the reservoir of the aliments. The anterior part is also marked behind with a lucid spot, smaller, narrower, and longer than the other, which I have sometimes fancied the œsophagus. The figure of the body presents nothing agreeable; it very much resembles the testicle of a cock. The sloth is represented supine, Fig. 7. pl. 3.; and the profile is seen, Fig. 8.

This animalcule forms no vortex in the water; which is not surprising, as it has neither the wheels nor the vibrating fibrillæ of the animals which perform this operation. It appears that the wheel-animal cannot advance a step, without fixing the trident to some adja-

cent ſubſtance. It is not the ſame with the ſloth; for it does not make frequent uſe of its hooked filaments. It never ſwims. It is ſpecifically heavier than the water. It turns round upon the ſand, or walks among it.

The phænomena of its death from the want of water, and of its reſurrection when water is ſupplied, are preciſely the ſame with thoſe of the wheel-animal. The motion is gradually loſt; the limbs are contracted and drawn into the body; the whole diminiſhes, is completely dried, and aſſumes a globular figure; Pl. 4. fig. 1. The reverſe of this takes place, when, upon ſupplying new water, the ſloth recovers life. As the wheel-animal can revive only a certain number of times; ſo is it with the ſloth. It ſeems, that although ſand is neceſſary for its reſurrection, yet it is not ſo eſſential as for that of the wheel-animal. The degrees of heat injurious to revived wheel-animals, are alſo injurious to ſloths; and it is the ſame with odours and liquors. Cold, however intenſe, does them no harm. Such are its ſimilarities to wheel-animals.

Sloths are much more rare than wheel-animals. It is not common, for five-and-twenty wheel-animals, to find above three or four ſloths. All ſloths are of the ſame figure, although not of the ſame ſize. I have iſolated ſeveral in watch-glaſſes, ſometimes with ſand, and ſometimes

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with pure water, meaning to fee how they propagated; but, inftead of multiplying, they all perifhed; in the firft cafe fooner, and in the fecond later, never attaining the fixth day in life.

The third fpecies of refurgent animals found in fand, is that of minute eels, very like the anguillæ of vinegar. This fpecies is much more rare, and it is not found upon every roof. The head and adjoining part of the body is very tranfparent, of a fhining filver colour; the tail is the fame; but the intermediate part of the body is a little dark, and is granulated upon the furface. The greater part of the tail is bent, and terminated by a very fharp point. The head, on the contrary, is obtufe; and, a little below its extremity, there is a mouth, which terminates a little canal ferving apparently for an œfophagus, and running through the length of the body; Pl. 4. fig. 2.

If the fand they inhabit is very dry, they are feen motionlefs, dried up, and generally rolled in a fpiral; but, when moiftened, they foon exhibit figns of life. The tail firft begins a gentle motion, bending and turning in different directions: then the head moves, and afterwards the reft of the body; fo that the whole animal quickly becomes animated. The confequence is, that the fame degree of humidity is not required to animate this fpecies as

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by the wheel-animal and the ſloth, which do not revive, unleſs completely immerſed in water. The eels do not change their ſituation much; they only extend, contract, turn, and bend themſelves. If the ſand is thoroughly wet, their agility and rapidity of ſwimming is as great as that of the eels in vinegar. They live a long time in watch-glaſſes, providing they have water; and, if there is ſand at the bottom, they hardly ever quit it, always moving about the grains, and touching them with the head, which would induce us to ſuppoſe they do ſo in ſearch of food; becauſe ſome more minute and delicate ſubſtances are taken in by the mouth, and paſſed to the œſophagus. I never obſerved them propagate, although kept long in the glaſſes.

When the water evaporates they die; but they reſiſt death longer than wheel-animals and ſloths. Sometimes a ſmall degree of motion is preſerved ſeveral minutes after evaporation. When dead, the figure of the body is changed; the length is contracted, the breadth is diminiſhed. Upon being moiſtened, they inſtantly reſume their original ſize, and their animation re-appears.

There are conditions eſſential for their reſurrection. When the eels are in ſand, a quarter of an hour is neceſſary to recall them to life; but when in pure water, there is a great difference,

difference, according to circumstances. If it is only the first or second time they revive, there is not much difference in the time required for their resurrection: but, in proportion as the number of resurrections encreases, the time necessary for resurrection always becomes greater: an hour at least is required, and sometimes this hour is not enough, for the fourth resurrection: for the fifth, there is still longer required; and so on for the rest. But the frequency of resurrection in pure water, as in sand, has its limits, like that of wheel-animals and sloths. The eels die for ever at the seventh, or eighth, or, at most, at the ninth resurrection; and, although placed in a humid situation, revive no more. I may add, that at each resurrection part of their agility is lost; so that the last is but a simple change, from immobility to languid contorsions of the members.

There, then, are three species of animals inhabiting the sand of roofs, of which nature has permitted the resurrection after death. Those three are the only inhabitants of this sand; at least, I do not think I have ever seen other animated beings there, having a permanent abode. They are not the only animals in the world that enjoy the privilege of resurrection; there are also others which possess it. Among those are the celebrated eels of blighted corn. All the

the world knows that Mr Needham is the author of this famous discovery. Examining the internal surface of blighted corn, he saw, with agreeable surprise, that it was composed of minute eels, which, upon being wet, acquired motion, and gave certain signs of life. This sudden resurrection, as he has thought, takes place when the ears are gathered still fresh and humid; if they have been gathered for some time, and have lost their humidity, it is necessary to macerate them, and this will not always be sufficient for the resurrection; it is even necessary that the eels which have come from the ear shall remain for a given time in the water. We also see, that when allowed to dry, they become motionless; and wetting them again, they recover motion and life. But, what chiefly surprised the author of this discovery was, after having preserved the blighted corn for two years and more, he observed, anew, the same phænomena upon wetting it.

The discovery was too wonderful for others not to endeavour to ascertain it. It has been found real by several good observers, such as the translator of Mr Needham's work, who speaks of this discovery [a]; by the Count Ginnani [b]; but particularly by the celebrated Baker, in his excellent dissertation upon *the eels of*

[a] Nouvelles observations microscopiques.

[b] Delle malattie del grano in erba.

of blighted corn [a]. Among other things, he has seen the resurrection of eels, taken from grain that had been dry during four years: this observation he made before Mr Folkes, then President of the Royal Society, and other friends. But he saw a resurrection much more wonderful, of the same eels, which was effected after a far longer time. In 1771, he had some blighted corn, which he had got from Mr Needham in 1744. He made some experiments upon this corn, and the resurrection of the eels succeeded perfectly, at the end of 27 years [b].

In short, there is not at this day any professor, any amateur of natural history in all Italy, who does not take pleasure in amusing himself, and the learned curiosity of his friends, with those admirable resurrections. For this reason, I judge it needless to stop to prove, by new facts, their reality, and to speak of the origin and generation of those eels; for we know that this, which is the most essential part of their history, has been amply elucidated by the learning and labours of Italians. I shall but relate the results of some of my little observations, which may not only serve as proofs of their history, but are analogous to those we have related of other resurgent animalcula. The external

[a] Employment for the microscope.

[b] Journal de L'Abbé Rozier.

ternal colour of a grain of blighted corn which has been kept ſome time, is like ſoot; if broken, the internal ſubſtance conſiſts of a dry whitiſh matter: when examined with the microſcope, its appearance changes into a maſs of long corpuſcula, ſhaped like eels; but thoſe eels are not only very much dried, but ſeem lifeleſs, and are ſo crowded and confounded together, that it is difficult to ſeparate without breaking them.

If this grain has been infuſed ſome hours in water, and the extremity adroitly cut off without injuring the inſide, and if it is preſſed with pincers, through the hole there is ſeen paſs a parcel of minute eels, juſt like a bit of paſte drawn into a thread: when let fall into water, they are ſcattered about, and falling to the bottom, appear extended as ſo many ſtraight lines, or a little bent, and remain in this poſition until they recover life.

I have attempted to know how long was required for their reſurrection, computing from the moment when the grain was wet; but ſo many varieties have occurred, that I have never ſeen the ſame thing happen twice. In ſome grains, the eels were reanimated in three hours, and even in leſs, and others in four or five hours; there were ſome which required twenty hours and even more, and ſome, complete days. The eels of the ſame grain were not all reanimated

mated at once; it sometimes happened that two days intervened between the animation of the first and the last. The whole eels of blighted corn do not revive; some are disfigured and lacerated; part of the whole are always of this description; but some, apparently entire and unhurt, preserve perfect immobility.

The heat and cold of the weather are not indifferent to their resurrection: heat accelerates it, and cold retards it: but there are also irregularities here.

There are symptoms which announce the animation of the eels; these I shall abbreviate. The first indication of life, is when the eels stretch themselves entirely or partially; they are no longer straight lines, as when they are dead, but the head and tail begin to curve, though the rest of the body continues a motionless thread in a straight line. Sometimes the two extremities do not bend; the middle of the body only becomes arched. Sometimes the one gently oscillates, while the other does not move; sometimes they approach each other until they touch at the extremities, and form a circle; sometimes one extremity rests upon the other, or glides over it; sometimes the whole body is rolled in a spiral, in wider or narrower volutions. Those bendings, arcs, oscillations, circles, glidings, volutes; those contortions, formed and destroyed, are repeat-

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ed at firſt with great languor; then in a more marked manner, and with more vivacity. This aſtoniſhing variety of motion, with others which it is needleſs to deſcribe, continues in the water during the whole time the eels live. Whence it appears they have nothing that may be with propriety called a progreſſive motion, which makes a difference between them and the other reſurgent ſpecies of animals. During life they never riſe in the water, nor do they crawl upon the ſides of the veſſel, but always remain at the bottom, where they are ſeen under the appearance of a nucleus, of a darker or lighter colour as they are more or leſs numerous.

If the water gradually fails, whether by evaporation or by taking it away, they gradually become lifeleſs; and when there is no more water, they no longer move. The three other kinds of reſurgent animals have the prudence to fly the places where the water dries; but the eels continue in the ſame place without moving.

In ſome hours, they become very dry, and tenaciouſly adhere to the bodies applied to them, ſo that it is difficult to ſeparate them without breaking; but, when wet, they ſeparate eaſily, eſpecially with the point of a needle. They ſoon ſoften, and twiſt themſelves, when it is ſeen they are of a gelatinous ſubſtance; and they cannot be touched with an iron inſtrument,

ſtrument, without being broken or injured. This at leaſt happens when they are in life. When they have been dead ſeveral days, they are ſtill very fragile, yet have more coheſion than one would think. They reſiſt the point of a needle, and do not even ſuffer from a drop of water let fall from a height upon them. When they have been dry for a quarter of an hour, they are reanimated by the contact of water, and become as vivacious as before. Urine, ſalt water, and vinegar, produce the ſame effect, although thoſe liquids are fatal to them in other circumſtances, as we ſhall ſee. When they have been dry ſome days, they require a full hour to recover life. If one has patience to wet, and let them dry, their death and reſurrection will be ſeen in this important limit; which is, that the oftener the humectations are repeated, the leſs the number of reſuſcitants will be, and the longer the time required for their reſurrection. I had a number of lively eels in a watch-glaſs the firſt time they revived: the thouſandth part did not revive the eleventh time; and the ſeventeenth there was not one. I have often repeated this obſervation, but always with the ſame ſucceſs; excepting that the reviving eels either went beyond the ſeventeenth reſurrection, or died before attaining it.

Wheel-animals, sloths, the minute eels of roofs, and of blighted corn, possess the property of resurrection, circumscribed within certain limits, beyond which it is lost. The body, to revive, must be entire. Eels cut into two or more parts, although often wet, and remaining long in water, never exhibit any sign of motion. When divided in two, they lose all sensation, after a slight universal vibration or convulsion of the body.

Like wheel-animals, I have subjected the eels to different experiments, and first to electricity, using Bevis' battery: those alive died instantaneously, and those dead at the time lost the property of resurrection. This did not surprise me; almost all were broken or disfigured by the traversing electric shock. In the blighted corn they have been subjected to the same experiment, but there was a difference in the results. When the grains had been previously macerated, few revived; if the grains were dry, many recovered life.

The eels are revived by salt water, urine, vinegar, when they have been a short time dry; neither are they so immediately killed by these liquids as other animalcula, for they will move in them several hours after immersion.

A vacuum does not affect their resurrection, whether the first time after proceeding from the grain, or after some resurrections; only the

resurrections

resurrections are not so soon accomplished as in the open air.

The heat of the sun or of the fire, at 140°, kills them in some hours: when raised to 144°, they are almost immediately deprived of motion and life. Heat more powerfully affects wet grains than dry. Sometimes the observer will have a number of eels from grains that have been exposed to 138° of heat; and sometimes the greater part are killed at that degree.

When freezing water becomes solid, the motion of the eels ceases. Cold 8° below 0 does not take away the property of resurrection; and when the ice melts, life re-appears.

Those who have never seen the eels of blighted corn, will find them designed, fig. 3, 4, 5, pl. 4, as they appear, when examined not by a very powerful magnifier, while swimming in the fluid. Seven blighted grains are represented of their natural size and figure, fig. 6, and three are magnified, fig. 7.

Plants are a kind of beings so analogous to animals, that we may excuse him who has defined them *ramose animals*. In the works of Vallisnieri, Buffon, Bonnet, and, lastly, of the Abbé Corti, may be seen the numerous and various traits of analogy between the two kinds of organised beings. The subject which I treat, presents a new analogy; for, as different animals revive after death, so do many plants

spring again after they have perished. I should depart from my plan, did I mean to say as much of them as I have said of animals; and I shall content myself with mentioning two, the Nostoc and the Tremella. The Nostoc, thus named by Paracelsus, is a terrestrial plant, which, from its sudden appearance in places where there was no mark of it before, was viewed by the ancients rather as a prodigy of heaven or of earth, than as a plant. So they have denominated it, heaven's flower, and earth's flower. It is seen in all seasons, but particularly in Summer, after heavy rains. Although it springs in every soil, it prefers meadows, arid lands, and sandy valleys. It is of a brownish green colour, and of an irregular figure, resembling a leaf carelessly folded. When separated with the fingers, some resistance is felt, such as when one tears a young leaf. If a sudden drought happens, the nostoc contracts and dries, remaining only a fine thin skin. If a sudden and heavy rain falls, it becomes green again, and resumes its original size. The nostoc is then, as Reaumur observes, who has furnished me with this intelligence, a plant of a singular kind, since it recovers life after being in a state which to others would be permanent death.

The Tremella is an aquatic plant, which enjoys the same privilege. Botanists have placed it in the class of Confervia. If in a vessel where the

the water fails, it dries and loſes its verdure; but if water be ſupplied, it ſoon recovers its original ſtate. Nature does the ſame with art. I have ſeen, from the beginning of July till the end of October, a ditch for watering land, covered fifty times with the beautiful verdure of the tremella, and ſeen it as often diſappear, when there was no water: I ſaw only at the edges and at the bottom, colourleſs hairs, which, the microſcope ſhewed me, were the tremella dry and dead.

But, what can be the reaſon why theſe ſpecies of animals and plants are thus privileged, in compariſon with a very conſiderable number of others, which, periſhing once, periſh for ever? Shall we perhaps repeat what we have ſaid upon the ſimplicity of the ſtructure of thoſe beings? But this does not ſeem to me well founded. There are many animals which do not revive, whoſe ſtructure is as ſimple, and even more ſo, than that of the reviving animals. Several ſpecies of the animalcula of infuſions, which ſeem compoſed of a ſimple aggregate of veſicles, enveloped by a fine membrane, are certainly more ſimple than wheel-animals, which are provided with veſſels, wheels, inteſtines, and ovaries; yet they do not recover life when once it is loſt. The ſimplicity of their ſtructure would thence appear an obſtacle to their reſurrection; for, when the infuſion has evapo-

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rated, the ſimple membrane of ſeveral ſpecies burſts: then they are diſſipated, and each is reduced to a maſs, without order or connexion.

The armed polypi are no leſs ſimple than the animalcula of infuſions, being compoſed but of a granulated gelatinous ſkin. If, therefore, ſimplicity of organization influenced the reſurrection of animals, the armed polypi would certainly be of the number. They ſeem ſo much the better adapted for it, as they continue in life, notwithſtanding every method uſed to deprive them of animation. It is demonſtrated, that they receive no injury after being turned ſeveral times outſide in, like a glove, or when cut aſunder. If the head is cut off, there ariſes a ſort of hydra with many heads, each of which receives food by a different mouth. If theſe new heads are cut off, there ſpring new hydras; and each head creates a polypus fit for the formation of new hydras. In ſhort, every particle of a polypus, the ſmalleſt fragment, developes and becomes a new polypus. If an animal ſo lacerated or mangled does not die, will it not appear very credible, that, only letting it remain dry, it will ſtill retain the faculty of reſurrection? But facts prove the reverſe. When the water evaporates, the armed polypus always dies, which happens equally, whether it is immediately expoſed to the air, or concealed in aquatic herbs.

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When the water is almoſt exhauſted, the arms are drawn within the polypus: it contracts within itſelf, and then loſes motion and life; which it never recovers, although afterwards copiouſly wet with new water. I ſpeak of the armed polypus: it is the only kind I have been able to find, and it is the ſmalleſt of Mr Trembley's armed polypi.

After polypi, and the animalcula of infuſions, according to my deſcription, the organization of the ſloth appears the moſt ſimple. We might ſay the ſame of the eels of tiles and blighted corn, which are two ſpecies of eels, eaſy to claſs from organization with ſo great a number of the inhabitants of fluids. Under the tremella, in the water, are often found minute eels, very like thoſe of tiles in ſize, ſhape, and ſimplicity of organization. I have frequently had the curioſity to let them dry by the evaporation of the water: they all endeavoured to conceal themſelves where the filaments of the tremella were thickeſt; and, when the evaporation was completed, they periſhed, remaining partly twiſted among thoſe filaments, and partly heaped upon each other. If immediately wet, they revive; but if a few minutes elapſe, they do not.

The eels of vinegar give the ſtrongeſt evidence of their vigour. Although they continue motionleſs when the vinegar fails, and are ap-

parently dead; they recover life and action if wet after a full quarter of an hour. Sometimes I have succeeded in reviving them after half an hour. I do not call this a resurrection; for, if it was such, I do not see why it should not take place anew, when wet with vinegar, after even a longer time. I shall rather say, they do not lose life so soon as the eels of the tremella, and several other aquatic insects, when left dry: when life is suppressed, it is preserved, and appears upon being wet with water.

I cannot find a greater simplicity in the tremella and the nostoc, than in many plants that do not revive. Let us throw a hasty glance upon the Truffle. That vegetable is more simple: no roots, tendrils, or fibres, internal or external, throughout a substance equally compact and uniform, only interrupted by veins similar to those winding upon some species of marble. In respect to other plants, not only terrestrial but aquatic, it has no analogous organization: yet truffles, after once drying in the air, do not revive when put in water.

These united facts shew how fallacious are the opinions of those who attribute the resurrection of animals and vegetables to the simplicity of their organization. To what principle then can we recur, since here we must proceed upon conjecture, rather than evidence, and the

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view of truth? I ſhall ſuggeſt an hypotheſis, without engaging to ſupport it. The experiments of Haller demonſtrate, that the vital principle of animals which have a heart, reſides in the irritability of this muſcle. His experiments are too well known, to need repetition. In animals which have no heart, it is almoſt probable, that the principle of their life reſides in the irritability of their muſcles; which being the caſe, if the ſtate of animals is ſuch, that the irritable nature of the heart and muſcles is deſtroyed, ſo as to leave no hope of it being repaired, it is clear that the animal not only dies, but muſt always remain dead: but if the irritability is ſuch, that, either by nature or art, it may be re-excited, it is certain that the animal ſhould paſs from death to life. It will not matter that it remains dead a long time, even for an age. The reader comprehends my idea. When wheel-animals, ſloths, and the eels of tiles, are deprived of water, their irritability is loſt, as is evinced by facts, and they die: when other animals have once loſt this irritability, they never recover it; but wheel-animals, ſloths, eels of tiles, &c. reſume at once their original life.

This theory explains why, in certain caſes, our animalcula loſe the property of reſurrection when expoſed to powerful heat, to penetrating odours; or when certain liquids, or electricity,

lectricity, act upon them. Those agents injure the muscular structure, which appears by the destruction of the body, and from the irritating force residing there.

This perhaps is the reason why frequent humectations injure resuscitant animals. I have particularly seen, in the eels of blighted corn, that the frequent humectations injured and altered the members.

We must thence conclude, that as irritability resides in the glutinous part of the muscle, this part has, in resuscitant animals, qualities very different from the irritable parts of other animals, although we are ignorant of this difference, because we are ignorant of what the difference in the gluten consists.

I am a friend to sincerity. From my experiments may be deduced a conclusion against this hypothesis. Irritability is recognised by its appearance; that is, when the muscular fibre is touched by any stimulant, it contracts and becomes rigid. I have often stimulated the muscles of the eels of blighted corn and of tiles, with an extremely fine iron point, attentively observing what happened. It always appeared, that the muscular fibre contracted a little when touched; but I must acknowledge, that I saw the same thing in the eels of vinegar, and in other analogous animals which have not the privilege of resurrection. There are even

even minute aquatic and terreſtrial worms, completely as irritable as the eels; ſince, if touched, however gently, they contract and ſwell, till they become twice as thick and ſhort.

The objection is therefore confined to this; There are ſome animalcula which do not revive, although they are as irritable, and even more ſo, than thoſe that do. This does not affect my hypotheſis; for it does not place the principle of reſurrection in the greateſt and moſt perfect irritability, but in an irritability which, after ceaſing, may be renewed by means of certain circumſtances, although it would otherwiſe appear to be leſs active than in other animals. If this hypotheſis does not entirely ſeem applicable to plants, at leaſt in what concerns irritability, ſince we know, that, excepting a very ſmall number, they have not this property, it is ſtill poſſible to apply it to their organization. In general, dried plants do not recover life; probably becauſe, in drying, they are ſo much injured, as to become incapable of imbibing the juices furniſhed by the earth, and of converting them to their own ſubſtance. Such is the cauſe of their periſhing, and of their total deſtruction. If the drying of plants does not occaſion this diſorder, and if the action of their organs revives, when ſoftened in water, and they reſume their original form, it is undoubted they will here recover their for-

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mer verdure and natural freshness. This may be the physical cause, why the tremella, the nostoc, and other vegetables revive.

I shall end this dissertation with some reflections upon those beings which we may, at pleasure, pass from life to death. When this idea is presented to the mind, we are astonished, because they are isolated beings: they form a separate class, and are adverse to the received ideas concerning animated beings. But, when it is proved by a train of innumerable facts, that all is gradated in nature, that those beings are connected to other beings, and consequently, the isolations exist but in the general system, our wonder should cease, or at least diminish, since it only arises from our ignorance of the relations which afford us the connexion of the class of beings possessing the prerogative of resurrection, with the other classes of animals. This is not the only isolated fact that has existed, and, at first, been viewed as an exception to general laws. To be acquainted with many, it is necessary to read the works of Reaumur, Trembley, and Bonnet. Those exceptions appear singular at first, because they are seen only in a single instance. A plant, an animal of a new genus, or which possessed peculiar properties, was the origin of those exceptions; but farther observation, and solid experiment, have shewn them adapted to several

ſeveral caſes, either in the ſame circumſtances, or under ſome modifications, and proportioned as people have applied themſelves to diverſify the number of ſubjects which have verified it. Some are ſo extenſive, that it is impoſſible to call them exceptions. This wonder, which affects the mind from ſomething new or extraordinary in the facts which occaſion it, gradually diminiſhes, and vaniſhes entirely. One or two examples will corroborate and elucidate my reflections upon reſuſcitant animals.

One of the moſt efficacious methods of deſtroying animals, is, to cut them into pieces. The experiment is common, and known. To ſay, that this was a method of multiplying ſome, would be to affirm what had a fabulous appearance. Yet it is the caſe with the armed polypus. And ſhall we ſay the diſcovery is bounded by this animal? The ſcalpel need only be applied to other animals, to prove the diſcovery extenſive: the reproductions of the earthworm, the boat-worm, the freſh water worm, ſome leeches, ſea-ſtars, ſea-nettles, &c. While art effects prodigies upon thoſe ſpecies, nature prepares ſimilar in ſilence; I ſpeak of the natural diviſion of the dart millepede, of ſeveral ſpecies of club, funnel, and bell polypi, and a great number of infuſion animalcula. We alſo find, that nature forms not only two animals from one only, but even four, and a

multitude

multitude ſo prodigious, that there are as many ſimilar animals as there may be atoms in the generating animal. But the wonderful progreſs of the diſcovery of the polypus, does not end there. It is a chain paſſing from vegetables to animals, and leads to man. The tremella is the link connecting animals and vegetables; it is a real zoophyte. It has been diſcovered, that the filaments of which it is compoſed, ſpontaneouſly divide, and that a complete plant ſprings from each diviſion. The polypus is joined to the tremella, and is united to a number of ſpecies dividing like itſelf, which in the ſame manner are linked with other ſpecies. But reproductions are not effected in the ſame way in all thoſe beings. One may reduce a polypus, a freſh water worm, a ſea ſtar or nettle, to the ſmalleſt fragments, and be certain that each fragment will reproduce itſelf. The earth and boat worm cannot be cut into pieces ſo ſmall, and reproduce itſelf. If a ſnail is decollated, a new head will germinate; but the head cut off, will not acquire a body. Water-newts, and frogs in the figure of tadpoles, recover their tails and limbs when deprived of them; but, if mutilated in other parts of the body, they periſh [a]. No warm-blooded animal is known, which reproduces itſelf when cut in pieces: but animals recover

[a] Treatiſe upon animal reproductions.

recover large pieces cut away. Duhamel has ſeen a ring of fleſh cut to the bone, reproduced in the thigh of a chicken.

Similar reparations are every day obſerved in wounds, in the cicatrices of wounded animals; and we have certain evidence of the reparation of the tibia in a man [b]. It is thus that the diſcovery of the polypus, which, at firſt, ſeemed to revolt ſo much againſt rules thought general, extends to ſo many links in the animal chain. But, has this ſucceſs ſo rapid, this advance ſo great, exhauſted the ſubject of reproductions? No, certainly; it will appear but little known, if we conſider how bounded the number of animals is upon which our experiments have been made, compared with thoſe upon which we may make them. The element of water is moſt favourable to reproductions. How many inſects, worms, reptiles, zoophytes, are there inhabiting the briny waters of the ſea, or the freſh waters of rivers, pools, marſhes and ditches, upon which there was never made an experiment, and which, from the great reſemblance they have, whether in the modes of life and propagation, or in ſhape and external appearance with the reproducing animals, are doubtleſs calculated to reproduce themſelves?

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[b] This fact is related by the celebrated Bernard Moſcati, ſurgeon at Milan.

The idea of hermaphrodifm, until the beginning of this age, has been viewed as a thing more chimerical than true. Nature feemed to depofe againft it; but in how many hundreds of animals have not the cares of modern naturalifts found it? This admirable property has paffed by degrees from one fpecies to another. The polypus is a perfect hermaphrodite, without fex; it multiplies not only by divifions, but by fhoots. The puceron of plants is lefs an hermaphrodite; it has a fexual diftinction; and although, during Summer, it multiplies without copulation, at the end of Autumn, it is obferved to copulate. Earth worms, fhell fnails, naked fnails, and many fpecies of fhell fifh are more or lefs hermaphrodites. They are, at the fame time, male and female, but infufficient alone. They give and receive, they fertilize and are fecundated. The difcovery of refufcitant animals is far from being as extenfive as that of reproduction, and of hermaphrodites; but this arifes lefs from the paucity of thofe beings, than from the fewnefs of philofophers who have entered upon that branch of natural philofophy. Lewenhock was the firft who drew the curtain from before thofe wonderful objects, by his wheel-animal. He feems furprifed at a fact, unique, and unexampled in nature; and indeed no one thought that there was in all the animated

world

world another animal possessing this prodigy. But, since the profound researches of more modern naturalists have made others appear, I doubt not that the number of those wonderful beings will encrease as the study is cultivated.

The shades seen in hermaphrodites are not observed in reproducing animals, and cannot be expected in the resuscitant. There may be a greater or less degree of reproduction: an animal may reproduce more or fewer organs: an animal may perhaps be more or less an hermaphrodite, if hermaphrodism is taken in a comprehensive sense: thus, both possibilities have been realised, as we see in the facts I have mentioned. But we cannot reason thus upon the animals reviving after death: we cannot say that an animal dies more or less, or that its resurrection is greater or less: death and resurrection are two indivisible acts; so much, that neither has any gradation in the animals hitherto known, and it cannot be expected in those we shall afterwards discover. It is not because those animals have no particular gradations, and are not connected with others of the same kind; but because those gradations are difficult to be observed. We have seen the state to which cold reduces some animals; we cannot call it death. There is then a thread of life, a lesser life, to connect resuscitant animals with the rest. The greater part of plants

lose their verdure during winter: they have little sap, and it is motionless; they cease to take nutriment, to grow, to multiply; all which produce complete inaction. How many animals beside insects do we see with a less degree of life, even without excepting warm-blooded animals, which have a similar degree, and, among them, birds! I am far from thinking with Guagin, in his description of Muscovy, "Populos quosdam in Lucomonia regione Russiæ habitantes, quotannis vigesima septima "Novembris die, ut solent hirundines et ranæ, sic et ipsos frigoris hyemalis magnitudine mori, postea redeunte vere vigesima "quarta Aprilis die denuo reviviscere."

We cannot at the same time deny, that man may sometimes be in a similar situation with animals overcome by cold: when immerged in water, we may say he hardly appears to be in life. I will not affirm with some physiologists, because there is neither pulsation nor respiration found in the body, the pulsation of the heart, and the circulation of the fluids, are suspended; but I would rather think with Haller, that those motions are only too faint and obscure, to appear or be externally perceived. Examples of this we have in some animals half-drowned, where a degree of motion is perceived in the heart and in the blood. Besides, the life of man and animals, when half-

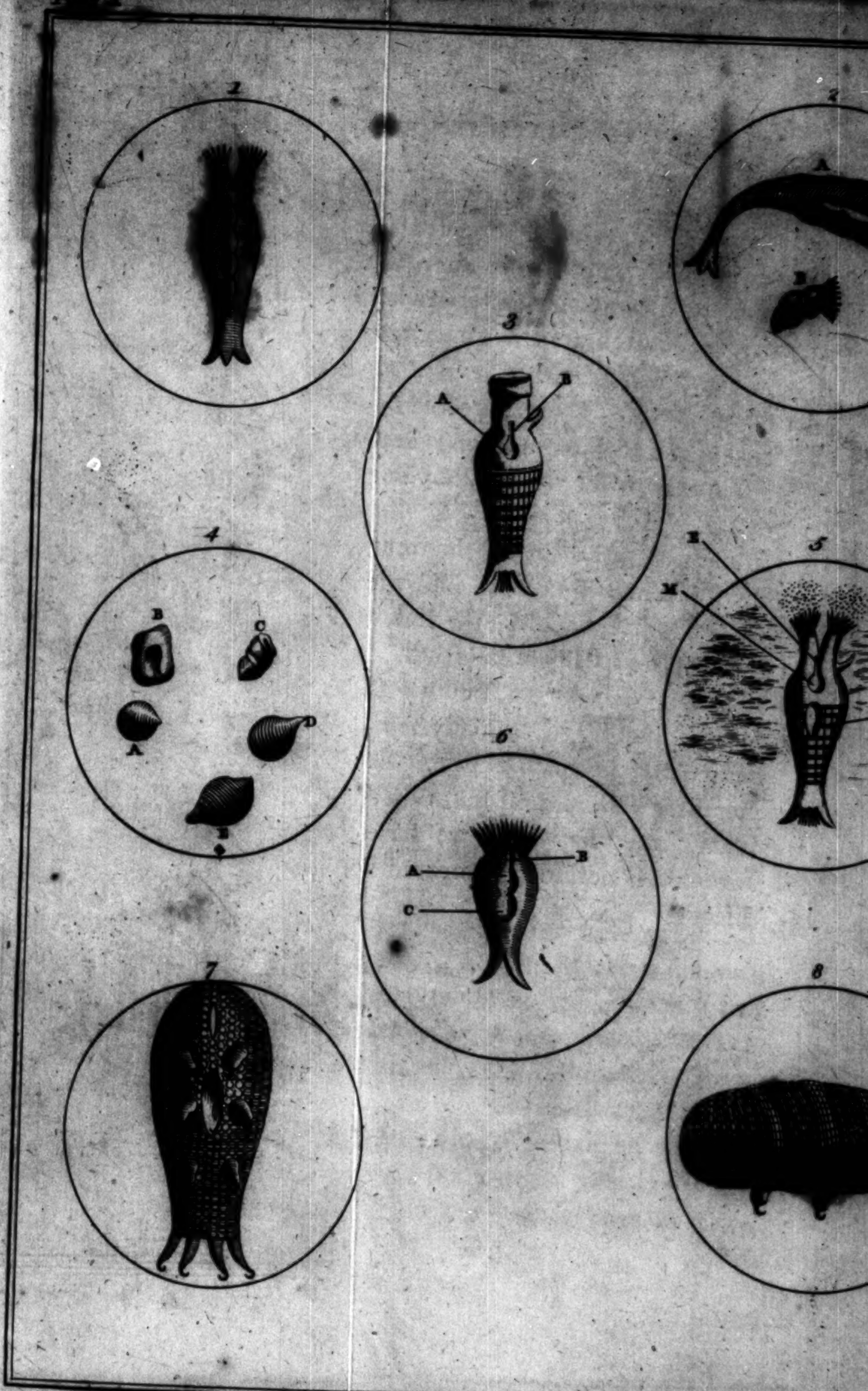
drowned

drowned or ſtrangled, cannot be more faint; and we may regard it as another point in the paſſage from the reſuſcitant, to thoſe animals which do not revive.

There are other two ſtates which are very ſimilar to the death of our animals. The one, the ſtate of the embryo in a fecundated egg which has not yet experienced the heat neceſſary for its expanſion; life is incomplete; there are but the rudiments or principles of life. The other ſtate is that of a chryſalis. Among inſects, when the caterpillar has loſt its natural form, it aſſumes that of a ſhapeleſs maſs, without the veſtige of feet or of wings: it ceaſes to feed; indeed to eat would be impoſſible, for the organs are wanting: it has no longer loco-motion; and we would really believe it dead, but for ſome inflection and contortion of which the poſterior part is ſuſceptible. The appearance of death is ſtill more ſenſible in the nymphs of many worms; they exhibit no ſymptom of motion or ſenſation.

Thus there are in nature ſituations a little ſimilar to the ſtate of reſuſcitant animals when they are dry. We ſee in the reſuſcitant animals, that this ſtate may be protracted or abridged at pleaſure. That torpid animals may never awaken from their lethargy; that the embryo may never expand in the egg; that flies and butterflies may never proceed from

the nymphs and the chrysalids, it suffices to keep them continually exposed to cold. The reverse will happen when they are exposed to heat. Probably there are gradations of connexion, more immediate and more direct, between the animals that revive, and those that do not. Life, however feeble and obscure, is always life; between it and death there is a distance as great as between entity and nonentity. An animal, whose life is suspended from an impediment to the mutual action of the solids and the fluids, and which is devoid of sensation, would be the link connecting the least degree of life with death. Such an animal is yet unknown: but we should not despair of finding it, so many being found which better link the chain of life. Let us only remember, that natural history is still in its infancy, and that what we have hitherto discovered, is nothing, compared with what is to discover.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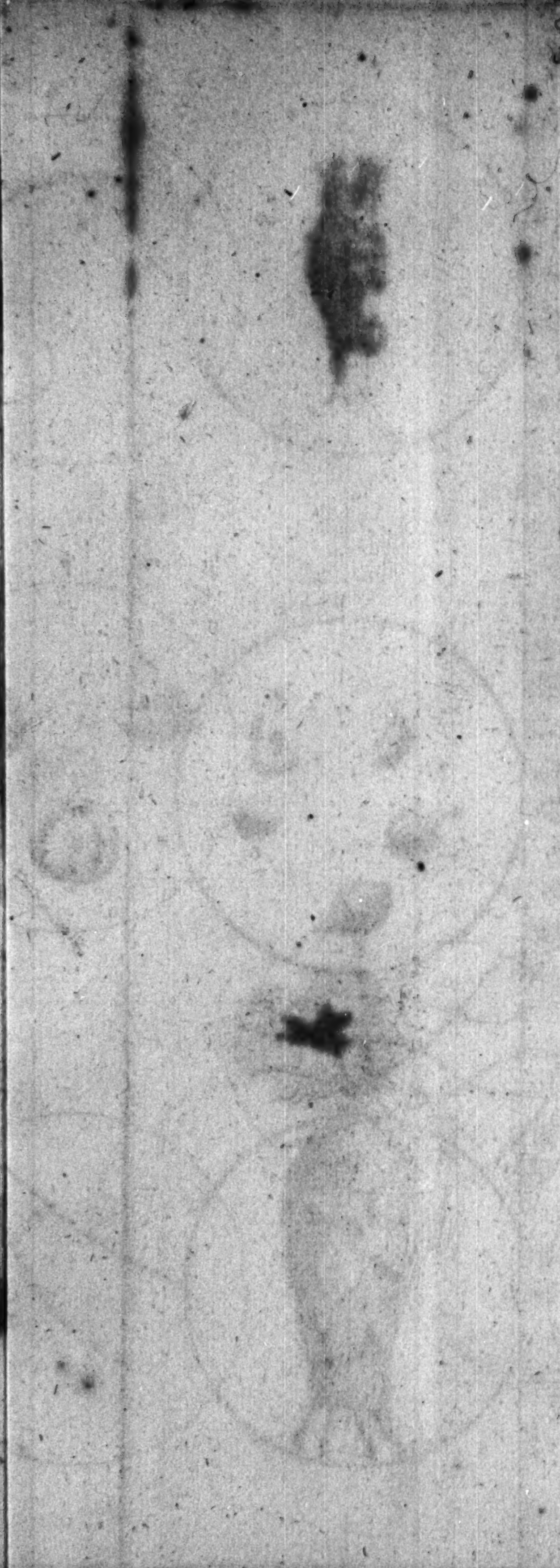
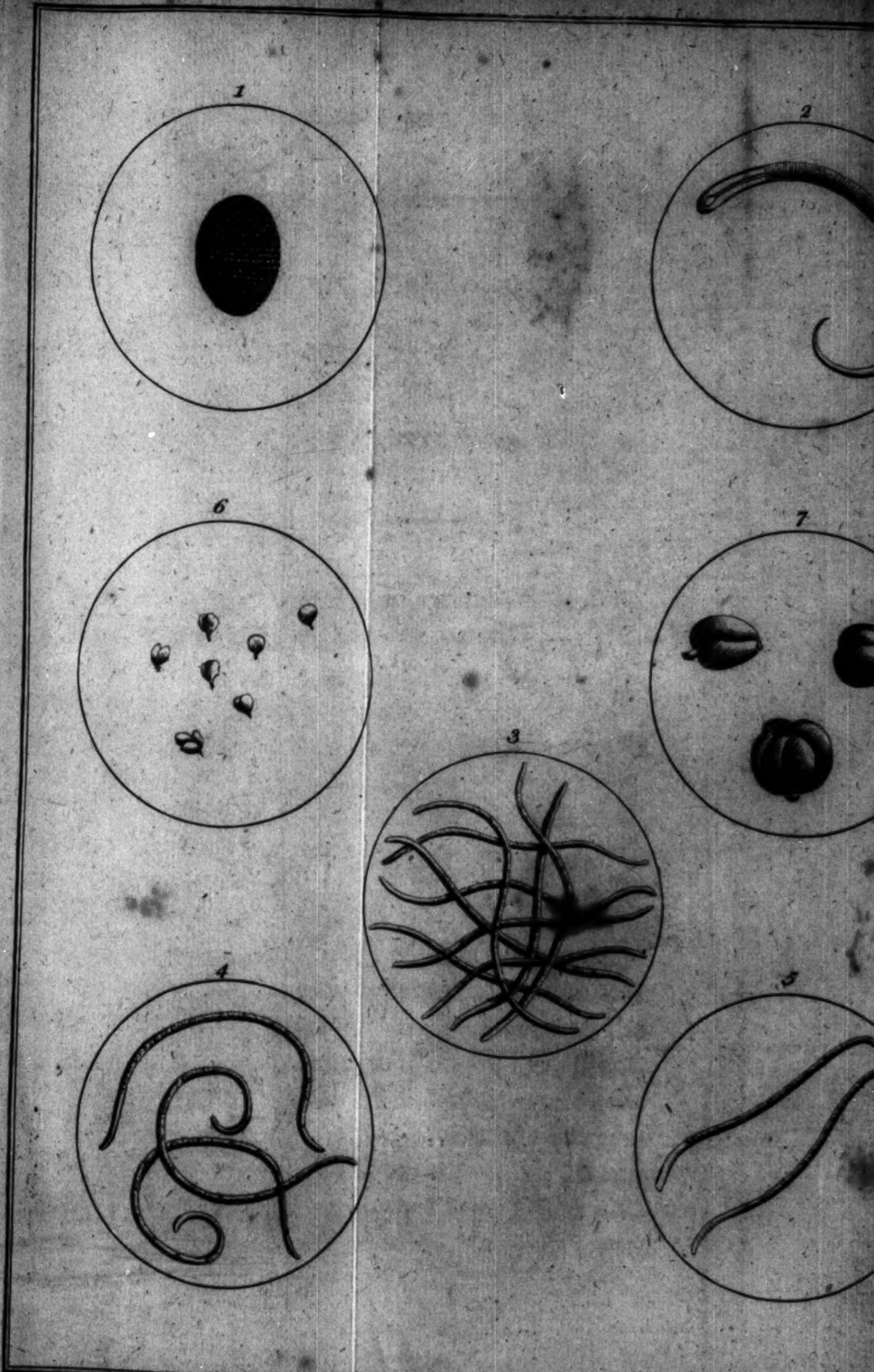


Plate 4.



EXPERIMENTS AND OBSERVATIONS

UPON

THE ORIGIN OF THE PLANTULÆ OF MOULD.

THE mould which I have examined, and intend ſimply and briefly to deſcribe, is that which ſprings upon apples, pears, melons, and gourds, beginning to ſpoil. It may be conſidered as of two kinds; the one very ſimple, eaſy to obſerve and deſcribe, the other complex and involved, which can only be explained by a generic deſcription. Let us begin with the former.

One ſpecies of this mould is without branches; each filament bears at the ſummit a globule, pl. 5. fig. 3. Another is ramoſe, but with this difference, ſome plants have a globule at the vertex of each branch, while others have it not, fig. 2. Thoſe globules I ſhall always term the minute heads of mould. There is one obſervation neceſſary to be made concerning them. Without the microſcope, they appear ſpherical,

and, even with it, they have the ſame appearance when viewed from above; but when examined below, that is, where the ſtalk is inſerted in the head, we obſerve that all, or the greater number, are ſhaped like muſhrooms; or, to ſpeak more philoſophically, they are real muſhrooms. Two filaments are repreſented with the globules, fig. 7.; the globules highly magnified, fig. 8. On pears there ſometimes grows a kind of mould, which is a real tree in miniature, univerſally adorned with ſpherical heads or muſhrooms. A ſtalk of this mould is repreſented, fig. 6. We muſt remark, that ramoſe mould is very often attached to vegetable ſubſtances, without any root: but the mould without branches, almoſt always has roots, originating from a round corpuſcle, whence the filaments forming the ſtalks of the mould are detached. It is a ſingular circumſtance, that as every root of mould has a greater number of ſtalks, the filaments from the root are proportionally more numerous. Upon tearing the mould from the ſubſtance where it ſprings, a degree of reſiſtance is felt, which ariſes from the roots being well fixed: when torn up, they appear very crooked, while the ſtalks, if they have not ſuffered from the impreſſion of the air, are very ſtraight. Many of the ſtalks are of an equal thickneſs, at leaſt they do not become

come much ſmaller towards the top. Fig. 3. will perhaps render this deſcription more plain.

Mould is at firſt of a moſt beautiful white; it then acquires a yellowiſh tint, and at laſt blackens; but the heads grow much blacker than the ſtalks and branches. The origin and encreaſe of mould is almoſt proportioned to the heat of the atmoſphere: it is never more apt to appear and vegetate ſuddenly, than during the heat of Summer: a ſubſtance which at night exhibits but a ſingle ſtalk, will frequently be covered in the morning, and then the mould has attained its full ſize and maturity. The height does not encreaſe ſo much as the thickneſs: the heads already black, are always of a larger ſize than thoſe ſtill young and white.

Mould is never ſo beautiful and vigorous as when it vegetates under ſome veſſel or receiver, providing the communication of the external with the internal air remains. The reaſon is evident. As the ſtalks are very fine near the ſummit, and bear at the vertex a round corpuſculum, oſcillating by its own weight; each filament oſcillates, as the ear of corn occaſions the oſcillation of the ſtalk. Thus we may eaſily perceive, that every breath of air, however gentle, will bend, break, and deſtroy thoſe moſt delicate filaments; which does not take place, when the ſubſtances to bear mould are put under a receiver. Beſides, the humidity of the

 ſubſtance

ſubſtance is better preſerved, which is a circumſtance moſt eſſential for the production and increment of mould. In the courſe of theſe obſervations, I have always made uſe of receivers. The injurious influence of agitated air is ſeen, fig. 1, which repreſents two quantities of mould with heads, as ſeen with the naked eye, when taken from under the receiver, and expoſed for a ſhort time to the action of the open air: their natural direction is loſt, and they turn to oppoſite ſides.

Several ſubſtances put in a ſituation to acquire mould, diſſolve into water, by which the adjacent plane is wet in proportion as they ſpoil; and it is preciſely upon this place the mould hitherto mentioned ſprings. From the ſame ſubſtance, there likewiſe exhales a leſſer degree of moiſture, which adheres to the inſide of the receiver, forming a pellucid aqueous veil; and the encreaſe is ſo conſiderable as to form large drops, which run down the ſides of the receiver in ſtreams, meandering among the moiſtened places: the ſame quantity of mould grows upon the ſtreams, as may be ſeen, if the receivers be tranſparent.

The other kind of mould we have deſcribed, is very complex, and always ſprings upon the vegetable ſubſtance itſelf, particularly upon gourds and moiſtened bread. When the putrefaction of theſe vegetables commences, a white

white thick covering of mould appears upon the surface, and in a few hours is a line high. When ripe, it is three lines high, or more. This is a species very different from that of which I spoke before, which, at complete maturity, is scarcely half a line high. We have already said, that each plant of this mould may be observed separately, and without confusion, and that the observation may be continued, distingishing each until maturity: but it is otherwise with the mould of which I now speak; such an observation would there be impossible. The immense number of plants when the mould begins to spring, the interweaving of the stems, and bending of the branches; the entangling and interlacing of the whole in an hundred different ways, completely prevent it: the sight is confounded among a multitude of slender filaments, which become in a greater or less degree a whole, deranged and confused as the vegetation of the mould advances: the substance can only be torn asunder to see its progress. When the stalks of this mould are pulled from the substance upon which they spring, it would appear they have no roots. From each of the numerous stalks, many twigs and branches proceed; but we should observe, that those subaltern productions are frequently of as great a diameter as the parent stalks from which they originate.

While

While the ſtalks vegetate and extend, there appear ſeveral groups of ſmaller ſtalks ſpringing laterally, with minute heads at the vertex. Theſe heads reſemble muſhrooms, partly globular, and, as the ſtalks riſe, the heads encreaſe, until they blacken and arrive at maturity. New ſtalks with heads, are, beſides theſe, ſeen to riſe, and the multiplication continues while the mould vegetates. This little foreſt is full of ſtalks; and its ſkirts are terminated by delicate points and black globules.

In this manner does mould ſpring, ripen, and die. We need not aſk whether it is a real vegetable; it appears to be ſuch too evidently, by the obſervations I relate. But theſe vegetables or microſcopic plants, do not poſſeſs two properties common to other plants. Ligneous and herbaceous plants, when expoſed to natural light, always tend to take a direction perpendicular to the horizon. They endeavour to attain it, and even to recover it, when taken away. The experiments of M. Bonnet are excellent, as may be ſeen in his work *Sur l'uſage des feuilles.* In mould, we do not ſee this tendency to perpendicularity; for, although many ſtalks are perpendicular to the horizon, this is not eſſential to their nature, and there are at the ſame time many other in a different direction. If a plant grows in the rent of a wall, although its firſt appearance is in a horizontal direction,

direction, it in a very short time rises perpendicularly towards the heavens. It is not so with mould. I have often cut a piece of melon, gourd or bread, into a cube: mould vegetated upon the four lateral surfaces, and the stalks had constantly every other than a perpendicular direction.

The other property which was discovered by the celebrated Genevese naturalist, is, the tendency of plants to turn towards the light. Beside the facts he relates, which are sufficient to ascertain the property, I have frequently observed it in legumes growing in infusions I kept shut up in a press. The plants always bended towards a chink, through which a very small ray of light penetrated: and, if I shut up this chink, and opened another in a different part of the press, the plants abandoned their original direction, to take this new one. I have endeavoured to learn whether this happened to mould, but could never discover that light had the least influence upon it. If ripe mould is shaken, a sort of black dust falls from it, which the celebrated botanist Micheli has thought to be the seed of the plant; but Dr Monti senior, a very eminent botanist likewise, has doubted the truth of this observation, and he rather inclines to think, that mould springs by a spontaneous generation.

Before

Before discussing this question, the object of which is so interesting, I think it proper for us to examine the place where the dust is found: to accomplish which, it is necessary to make a brief analysis of the heads of mould: this we can do only by examining and seeing it ripen. Before the heads are ripe, they are of a whitish or yellowish colour, the surface very smooth: upon being broken with a fine iron instrument, they seem to be membranaceous, and full of a granulated substance: if, instead of being broken, they burst, sometimes a number of minute round seeds come out. These are found both in the spherical and fungiform heads.

When the heads blacken, the appearance changes; the surface seems unequal; it is lacerated in several places, and resembles a parcel of black rags. When opened, a number of seeds is seen; but young mould has white seeds, and old or ripe mould has black. When the heads are moistened, the seeds are seen more distinctly, and in greater abundance. Upon contact with the fluid, or a little afterwards, the heads burst, and scatter around a cloud of seeds; so that I may affirm, without danger of exaggeration, there is a million in each head. The unripe heads do not open in this manner upon being wet; they remain entire. It must be remarked, the ripe heads are not totally decomposed. Where they are round like

like mushrooms, there is a little head in the centre, which continues to adhere to the stalk: this is cinder-coloured, with a degree of transparency, and does not appear black like the exterior: it is difficult to detach it from the stalk; but, with a small degree of pressure, a little jet of seeds is raised from it, resembling those I have described: the central head then becomes a dry and empty skin.

If the heads, when black and ripe, are opened by means of water, such a quantity of seeds issues forth, as to adhere in great numbers to the plants, in particular to the stalks; so that one would suppose the exterior composed of seeds alone, were they not seen in a different state before. One of those deceitful appearances is seen, Fig. 9.: it is intended to represent two plants of mould, one of which is entirely covered with seeds; the head is tumid, and likewise in a great measure covered with seeds. Fig. 4. represents two stalks of mould with heads: the whole seeds of the one are exposed, and another is perfectly covered by the integument; the third is entirely covered.

A quantity of those minute seeds constitutes the powder blackening the hands, when the mould is manipulated; and they are considered as real seeds by the celebrated Florentine botanist. To ascertain the truth, he had recourse

to

to a method apparently decisive, which was, to sow the dust. He strewed it upon some vegetables, and saw them covered with plants of mould. But the Bolognese professor has repeated Micheli's experiment, without finding it so conclusive; the vegetable substances being equally covered, although no dust was put upon them. Thus, the question remained undecided; for I do not know that any other person has attempted to resolve it.

Perhaps I shall be taxed with presumption, when I say I have been able, by means of experiments analogous to those of Micheli, to ascertain the fact; but experiments much more numerous, more diversified, and more connected with each other. The consequences I shall venture to publish. I took two pieces of moistened bread, as similar to each other as possible, and from the same loaf, so as to be perfectly equal. I endeavoured to attain the same equality in all the rest of my experiments. I strewed one of the pieces with dust taken from a quantity of ripe mould heads, so that the surface was slightly blackened by it: the other piece was untouched, in order to compare the production upon each. This was done in Summer. Upon the following day, the *sown substance*, but, for brevity, I shall observe, that by this expression I mean, the substance of whatever nature covered with dust, and

by

by the word *unſown*, the other vegetable ſubſtance not covered with duſt: upon the ſown ſubſtance, I ſay, next day appeared a ſhade of mould; whereas upon the unſown, there appeared none. Before the third day, both ſubſtances were covered with mould; but the mould of the ſown ſubſtance was almoſt double the height and thickneſs of that upon the other. Both ſpecies of mould were of the ſame kind, and perfectly ſimilar to that which had produced the duſt. Upon the fourth day, the mould of the unſown ſubſtance, although preſerving the original thickneſs, was equal to that of the other in height: it was ſtill higher upon the following day, but afterwards continued to become thinner. I repeated thoſe experiments eleven times upon moiſtened bread, and the effect was, that the mould twice became equally high and thick upon both ſubſtances, and nine times it was higher upon the unſown ſubſtance, than upon the other: it conſtantly ſprung firſt upon the ſown ſubſtance.

Having collected a great quantity of ripe duſt, I thought of varying the quantities, by ſcattering different portions upon moiſtened bread: the conſequences were new. When the quantity of the duſt was very ſmall, there was almoſt no difference in the height and thickneſs of the mould; but the thickneſs encreaſed, if a greater quantity was ſown. It was

was never ſo thick as when liberally ſcattered over the bread; but, in proportion as the thickneſs encreaſed, the height diminiſhed. Thoſe experiments were repeated again and again, upon apples, pears, and gourds; and the reſults were all, in a certain degree, more or leſs ſimilar to what I relate.

The conſequences that may be deduced from thoſe experiments, are theſe. 1. Sowing the duſt accelerates the production of the mould: 2. The thickneſs is encreaſed: 3. The height is leſs. Conſidering theſe facts with reſpect to my object, it ſeems to me, that the ſecond proves the duſt to be the real ſeed of the mould; ſo that ſcattering the duſt occaſions the more abundant production of the mould. If the thickneſs encreaſes with the encreaſe of the quantity ſcattered, it is natural to think, that the ſuperabundance of mould upon ſown ſubſtances is an effect of the duſt, or rather of the minute ſeeds ſown, and that all, or moſt part of the mould, originates from them. This being the caſe, it is not ſurpriſing that the mould upon ſown ſubſtances is not ſo high as upon the unſown; for, the plants being more numerous, each cannot imbibe the ſame degree of nutriment from the ſown ſubſtance, as may be derived from that which is not ſown, where the plants are leſs numerous; which is alſo the caſe with other plants, as they are ſmaller and

ſhorter

ſhorter in proportion as they are crowded. The firſt conſequence deduced from the facts demonſtrates, that the production of ſown mould is more early than that of unſown. I have thought that this might happen, becauſe the ſown ſubſtances ſooner ſpoil; ſince it appeared that the duſt germinated earlier, if the corruption of the ſubſtances advanced, as I before remarked.

I diverſified thoſe experiments. Sometimes I covered an half, ſometimes two thirds, or one, of a ſlice of bread, an apple, a pear, or a gourd, without touching the other half: the half, two thirds, or one third, were juſt in the ſituation of the ſown ſubſtances. I likewiſe made another experiment. After covering half a ſlice of bread, an apple, or a gourd, with duſt, I applied the ſurface ſown to another ſurface ſimilar, but unſown, leaving both in this ſtate for ſeveral days. Upon the whole ſown ſurface was ſeen a veil of mould, the vegetation of which had ceaſed, becauſe it was ſpoilt by the ſubſtance applied; but there was no veſtige of mould upon the unſown ſubſtance.

These laſt facts concur in corroborating the idea, that the duſt is the real ſeed of mould, becauſe the mould produced upon the places ſown, was exactly of the ſame ſpecies with that which produced the duſt. Notwithſtand-

ing all those plausible and repeated experiments, I was not satisfied. Is it not possible, said I to myself, that this dust only renders the soil more fertile, so that it will produce a greater quantity of mould, as the earth, fertilized by foreign matter, will produce a greater number of plants? This was certainly not impossible; and, wishing to proceed with philosophic strictness, I judged myself obliged to realize or destroy the possibility: for which purpose, I thought that I should cover the substances where mould springs, with dust taken from different vegetables, different earths, and other matter volatile from the extreme minuteness. It appeared to me, that if this dust could contribute to render the substance more fit to produce mould, it did not alone possess this advantage. The roots, stalks, and green heads of mould, were not spared. I dried and reduced them to a very fine powder, but without effect. The greater part, instead of giving that abundance of mould, deprived the sown substances of the faculty of producing it; and those which did not prevent the production, diminished the quantity of what the unsown substances used to produce. All these united facts seem to prove, that the minute grains proceeding from the heads of ripe mould, are real vegetable seeds.

During

During thoſe experiments, I was curious to learn, whether the ſeeds would germinate when ſown upon ſubſtances that did not mould. I ſowed a certain quantity upon hard bodies, as glaſs, metals, ſtones, &c.; alſo upon blot-ſheet and writing-paper; upon cotton, ſponge, &c. All thoſe ſubſtances were kept moiſt, to ſee whether they would mould; but there appeared no mark of it, excepting ſome filaments I ſaw upon ſponge. To expand, certain circumſtances are required by the ſeeds of mould, and thoſe are only found in certain ſubſtances.

The minute ſeeds, or duſt of mould, poſſeſs the peculiarity of reſiſting a degree of heat which no other ſeeds can ſupport, without loſing the faculty of germinating. After boiling the ſeed in water, I poured out the water, then become black, upon ſubſtances that mould, and where the mould, according to cuſtom, grows thicker than upon the ſubſtances unmoiſtened. I did the ſame with ſome duſt expoſed to a much greater heat, ſuch as that of a hot chafing-diſh, and have found, that as this heat does not deprive the ſeeds of the property of reproduction, neither does it alter their ſize and figure; as I have convinced myſelf by examination with the microſcope, before and after expoſing them to the heat.

But, does the mould, which ſprings without being ſown, and by the care of nature a-

lone, upon an infinity of fubftances difperfed here and there, alfo derive its origin from the duft we may fuppofe diffeminated through the air, and upon terreftrial fubftances? If natural and artificial mould are of the fame fpecies, and if the artificial is produced by the duft of the natural mould, I cannot fee why the laft fhould not derive its origin from the fame principle, efpecially fince it is demonftrated, that no other part of the mould, as the roots and ftalks, aid the reproduction. The hypothefis, fuppofing that this duft is invifibly fcattered through all, and gives exiftence to a great quantity of natural mould, is one of the moft reafonable hypothefes in philofophy. If each head of ripe mould can furnifh a million of feeds, as we have feen, and if each fpot of mould contains a prodigious number of heads, it is clear, that in fome years, the duft fhould be extremely multiplied; particularly, from its levity and finenefs, it may be univerfally fpread.

We have certain evidence that feeds may be kept a long time, without lofing the faculty of germination. My illuftrious friend M. Bonnet told me a fingular fact. In 1748, corn was carried from Sicily to Geneva, and lodged in the magazines of the republic. Some individuals fowed part of it in a walled garden, 1771. Notwithftanding the length of time,

it

it vegetated very well, and nearly as thick as in common with this grain. The wonderful minuteness of the seed of mould, seems to adapt it well for long preservation; but I have given a cogent proof of this. Heat is undoubtedly one of the most powerful agents in depriving seed of its germinating faculty. In my tract upon infusion animalcula, it has been seen, that the number of seeds that can support the heat of boiling water, is very small: and, if the very singular case of M. Duhamel is recollected, where seeds germinated after experiencing 235° of heat in a stove, it is demonstrated, that those of mould are not destroyed by a degree infinitely greater. This being the case, it is not absurd to think, that seeds which resist the injuries of weather, may preserve their fecundity for ages. It is thus very easy to comprehend how immense the quantity of this vegetable should be, since its seed multiplies so much, and is preserved so long, and that it should be so largely disseminated over all terrestrial substances, as always to be ready to germinate, when the circumstances, essential for this, occur.

Thus, the first of Signor Moscati's doubts concerning the universal generation of mould, is resolved, which induced him to think it was produced by a spontaneous generation. The other doubt is equally removed, from which he

deduced, that he had seen moulding substances, mould the same after being boiled: but, if the seed does not lose its virtue of germinating, after being exposed in a burning chafing-dish, it is not surprising that it is preserved at a degree of heat so far inferior as that of boiling water.

Although the substances upon which I made my experiments, were constantly kept under receivers, which was done with the view of having more luxuriant and beautiful mould, the communication of the external with the internal air was not interrupted. I wished to discover what would happen if the communication was cut off; and, first, what would be the consequence of lessening it. Moulding substances were put into very large glass vessels: the necks of the vessels were then drawn to a point, by means of the blow-pipe; and the point could be made of any degree of fineness. I had vessels, into which a stream of air no larger than a hair could be admitted; a little more in some, and still a little more in others.

All the confined substances moulded in a certain time. In the vessels with a very small aperture, I remarked two circumstances; that the vegetation was slower, and the mould did not grow so high, as when the aperture was larger.

The

The vegetables within, always perſpired ſo much, that the vapour collected at the apertures, and obſtructed them, particularly when they were very ſmall. This inconvenience may be corrected by ſucking out the moiſture; but, if this is not obſerved, mould will not grow, or hardly at all, in very ſmall veſſels.

My curioſity being ſatisfied with regard to this, I paſſed to another thing. I excluded the external air entirely, which was eaſily done by an hermetical ſeal. All the veſſels were not of an equal ſize; ſome would contain ſix pounds of water, ſome only one, and others but a few ounces. This variety of ſize affected the mould. In the largeſt, although of the ſame capacity with thoſe in which the mould was expoſed to the air, it did not germinate in the ſame abundance, and was later of appearing: the mould in thoſe of a middle ſize was ſtill later, and more rare: its ſtate was worſt in the ſmalleſt veſſels; none appeared in ſome, and in others a ſlight ſhade was ſcarcely ſeen.

After veſſels of three different ſizes, containing vegetable ſubſtances, were hermetically ſealed, I put them during ſeveral hours in boiling water. In the ſmalleſt veſſels there was no mould, in the middle-ſized a little, and plenty in the largeſt.

I put moulding ſubſtances in vacuo: the reſult of ſeveral experiments was, that during the

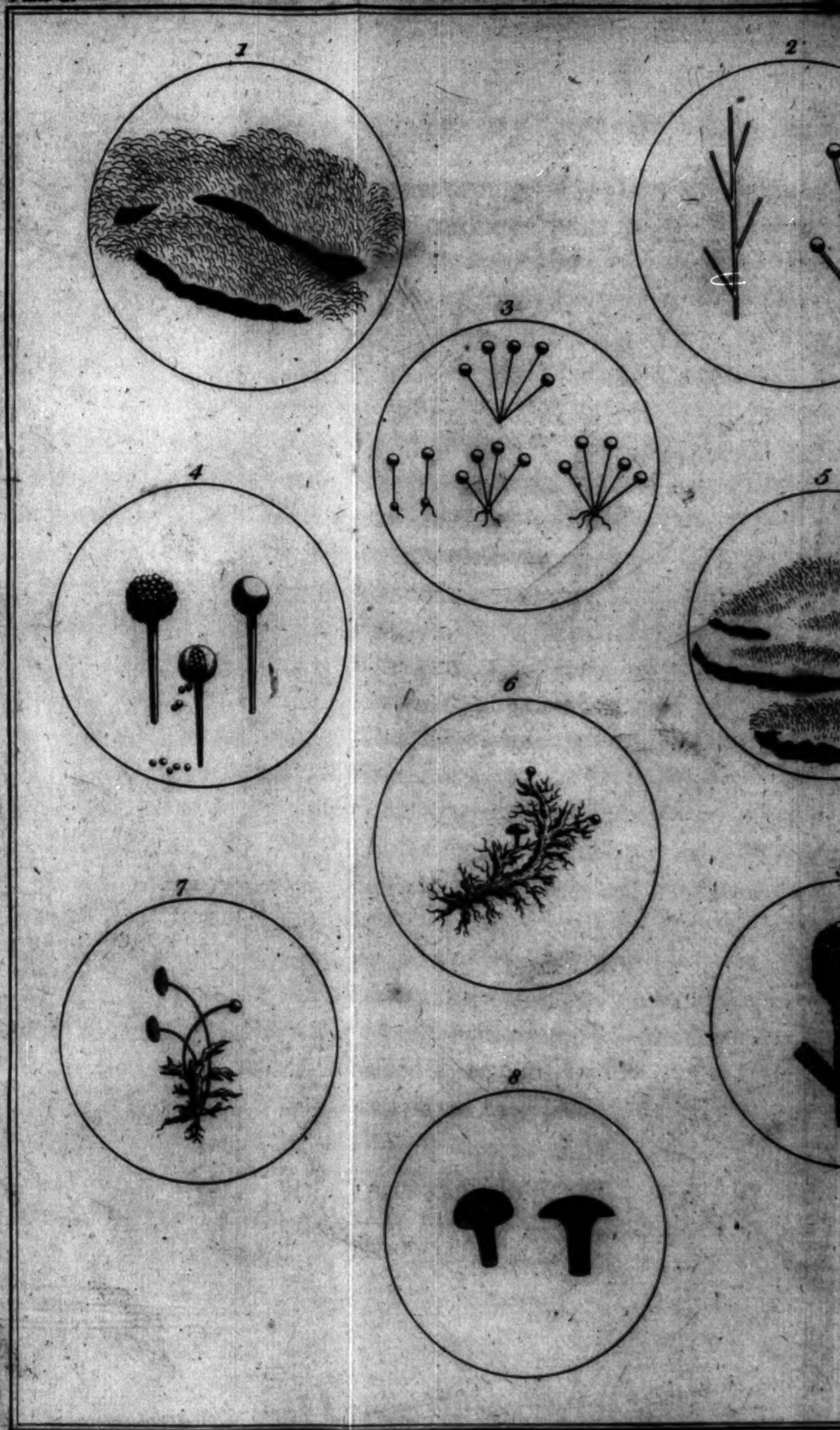
time, which was always ſeveral days, the ſubſtances were there, if by chance a portion of air inſinuated itſelf into the receiver, a quantity of mould appeared, which came to maturity without riſing high. When the air was entirely exhauſted, none ever appeared. Some plantulæ of mould are repreſented, fig. 5. pl. 5.; they were produced in a receiver upon an air-pump, where the vacuum was not complete.

Theſe three different experiments prove, that the plantulæ have the ſame relation with air as other plants, only it ſeems to me that air is not ſo neceſſary to them; for, when a thread of air entered the receivers, ſome mould vegetated, though the leguminous ſeeds within could not vegetate in that ſituation. Neither do ſeeds germinate in veſſels hermetically ſealed, although, as I have ſaid, vegetables will mould there. The ſimplicity of mould undoubtedly contributes to render the preſence of air leſs eſſential: in the ſame way as animals, that are leſs compound in the ſcale of organization, may be produced, and exiſt in a leſs quantity of air than is neceſſary for us.

M. Bonnet, in his judicious reflections upon mould, queſtions, whether we are ſure that all mould belongs to the vegetable kingdom, and whether there may not be ſome ſpecies approaching the mineral, or may be the connexion between the two kingdoms. This is not

impoſſible,

Plate 5.



impoſſible, when we conſider how prodigiouſly diverſified this claſs of beings is, and how little its ſpecies are hitherto known; if we eſteem that foſſils occupy the loweſt place of the vegetable kingdom.

Beſide the mould deſcribed, I have not neglected to throw a glance upon many different ſpecies; and I muſt ſay, that I have found in all, very decided characteriſtics to judge them real vegetables. I muſt likewiſe ſay, that the various kinds of mould I have obſerved are very few, compared with the immenſe number yet remaining to be examined; for there is almoſt no ſubſtance, animal or vegetable, which is not in certain circumſtances liable to mould. Thoſe who attach themſelves to this branch of Botany, will have ſufficient uſeful practice; and perhaps they may ſucceed in diſcovering the link connecting vegetables with minerals, which will do Philoſophy a moſt important ſervice. For my own part, I ſhall be content if, among other things, I have reſolved the queſtion concerning the real origin of the moſt common mould. The ſubject has not before been completely diſcuſſed; and it has led ſome perſons into the ancient and dangerous error of Spontaneous Generation.

EXPE-

EXPERIMENTS

UPON THE

REPRODUCTION OF THE HEAD

OF THE

TERRESTRIAL SNAIL.

BY

CHARLES BONNET.

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LAND SNAIL.

I PUBLISH my experiments upon the reproduction of the head of the snail, only to afford an additional confirmation to the Abbé Spallanzani's beautiful discovery. It is well known how much this discovery has been disputed beyond Italy, and particularly in France. There are naturalists of my acquaintance, who, after decapitating hundreds of snails without success, have thought themselves at liberty to conclude, that the Italian observer had allowed fallacious appearances to impose upon him. One of those naturalists, writing to myself, did not hesitate to reproach me for having inserted an account of the imaginary discovery in the *Palingenesie*, and for reasoning upon it, as upon a fact the full ascertained. It will be thought with jus-

EXPERIMENTS

UPON

THE REPRODUCTION OF THE HEAD OF THE LAND SNAIL.

MEMOIR I.

I PUBLISH my experiments upon the reproduction of the head of the snail, only to afford an additional confirmation to the Abbé Spallanzani's beautiful discovery. It is well known how much this discovery has been disputed beyond Italy, and particularly in France. There are naturalists of my acquaintance, who, after decapitating hundreds of snails without success, have thought themselves at liberty to conclude, that the Italian observer had allowed fallacious appearances to impose upon him. One of those naturalists, writing to myself, did not hesitate to reproach me for having inserted an account of the imaginary discovery in the *Palingenesie*, and for reasoning upon it, as upon a fact the best ascertained. It will be thought with justice,

tice, that those reproaches did not weaken the confidence with which the ability and the sound reasoning of the celebrated naturalist of Reggio had inspired me. Besides, he had communicated to me, in a course of correspondence, the interesting history of his experiments at length: and it was easy for me to judge, only by a trial of the facts that the judicious observer had seen and reviewed, the new wonders he laid before me, and which, in a short time after, he presented to the public in an Italian tract, which appeared 1768, and was, the same year, translated into French. But, since the author did not detail, in that treatise, the precautions he had taken to secure his discovery against all dispute, I requested him to publish an account of his method, which he did in a letter from Modena, 11 September 1769, and it was printed in the *Avant Coureur*, 30 October. This letter, so well calculated to destroy every doubt, has only done so in part; some remain, and people continue to oppose to the experiments of Padua, others which they judge contradictory to, or apparently confuting them. This conflict of experiments, which has continued nine years, induced me myself to repeat the experiment of the learned professor of Padua. I proceed to give an account of it; the impartial public will judge, from the details, of the confidence it deserves.

The

The ſpecies of ſnail upon which I operated, is of a middle ſize, and frequent in the fields, or in gardens, after a rainy day: then, numbers abandon their dark retreats, and in a ſhort time one may collect hundreds. The ſhell of ſome is yellow, or yellowiſh: upon that of others are circular black or brown faſciæ.

It is not an eaſy matter to decapitate a ſnail. The moment it feels the inſtrument, it ſuddenly contracts into the ſhell. Thus, it will eaſily be ſeen, that it may happen we think it is decapitated, when only a portion of the integuments is taken away. To avoid being deceived, I have uſed more precautions than one. The ſnail is allowed to extend as much as poſſible; and an additional extenſion is procured, by immerſing the animal in water. The inſtrument is preſented to the origin of the head, ſeveral times before ſtriking the blow; and I only eſteem the operation completely performed, when the head is obtained entire, with the four horns well diſplayed, and the mouth, which may always be recogniſed by the lips marking the opening. The head, as it appears ſome inſtants after ſeparation from the trunk, is repreſented a little magnified, Fig. 1. pl. 6. The two large horns, *g g*, are a little contracted. The ſmall horns, *p p*, are entirely contracted within themſelves. The mouth,

mouth, *b*, is closely shut. The lips are very evident.

A sharp-edged knife seems to me more fit for this operation than a scalpel. Scissars are still less convenient than a scalpel. I always have made the cut perpendicular to the axis of the trunk.

Immediately after the operation, the snail retires far within the shell, and in general does not appear again. At this time, a large portion of that viscous humour, of which it has so much, is diffused. If, at the same time, the severed head is viewed, some motion is perceived in the horns, chiefly in the large; but the motion very soon ceases, and I have in vain tried to renew it, by stimulating the head near the origin with the point of a scalpel. The four horns contract themselves to a certain degree, immediately after the operation, the small contracting more than the large.

By a very simple method, one may ascertain whether the operation is complete. This is, to immerse the decapitated snail in water. It is not long of leaving the shell: it extends as much as before the decollation; and then it is easily seen whether the trunk is entirely deprived of the head [a]. The anterior part of such

[a] However, it may happen that it does not extend so much as one may wish, or as is necessary to judge of the progress

ſuch a trunk, drawn from the life, is repreſented, fig. 2.; and the profile, fig. 3. It may be remarked, that the fleſh is ſtrongly contracted, to cloſe the enormous wound.

The viſcous matter copiouſly diffuſed by the ſnail after decapitation, produces at the opening of the ſhell an operculum, which completely ſtops up the entry: it is very thin, and of a whitiſh colour. Frequently, two of thoſe opercula are formed, one ſituated above the other. Sometimes there are three: the exterior is near the edge of the ſhell, the interior more or leſs within it.

The decapitated ſnail can reproduce ſeveral of the opercula; but the viſcous matter is by degrees exhauſted, and the ſhell remains open, or nearly ſo. As the animal has no method of feeding while deprived of its head, it cannot continually repair the loſs of this kind of varniſh. It inſenſibly becomes emaciated, which is ſeen by the diminution of ſize, and a ſort of tranſparency to be remarked in its interior. I have ſometimes been aſtoniſhed at the number of opercula that have ſucceſſively

 been

progreſs of the reproduction; but it is only neceſſary to take the ſhell between the fingers, after the animal has been taken from the water, and it will ſoon extend as much as poſſible. Great care muſt be obſerved to avoid touching the ſnail; becauſe, upon the moſt gentle contact, it retires into the ſhell.

been reproduced by decapitated snails. All snails do not produce opercula; but the number of those whose shells continue open is very small.

I have included my decapitated snails in boxes. Some remained at the bottom; others reached the sides, against which they applied the opening of the shell; others ascended higher, and attained the covering, where they fixed themselves in the same manner. These were apparently the most vigorous, or had suffered least from the operation.

When I wished to learn from week to week what was the real state of my decapitated snails, I had only to take away carefully the operculum or opercula stopping up the opening of the shells, and then immerse them in very limpid water. They are thus forced to proceed from the shells sooner or later. It has happened oftener than once, that they did not appear till several hours after immersion. This method, to which I have always had recourse to know the state of my snails, seems to me the best. They extend to the utmost limits in the water; and the anterior part is then so completely exposed, that nothing can escape the eye of the observer. They endeavour to leave the water; and gradually attain their purpose, if the depth is not too great. They crawl slowly over the bottom, and along the sides of

of the vessel, and advance until they reach a dry part. Then they fix themselves; and, to force them to appear again, it is necessary to re-immerge them in the water. Although absolutely deprived of a head, they advance forward as if they had one, only their progress is a little slower.

At first, I decapitated only a dozen of snails. This was 8 May 1777. I repeat, and I cannot too often repeat it, for I wish to anticipate the most trivial doubts, that I have esteemed the decapitation complete, only when I had upon my tablet the head entire, or attended by all its appendages. All the heads, in this manner severed from the trunk, have been ranged together upon one side of my tablet, and remain there at the moment I write this [a].

I now proceed to give a sketch of the wonderful reproductions which were effected by the snails before my eyes. I will not enter into a mintute detail: it is unnecessary: my purpose only is, to prove the reality of the reproductions, against the detractors of the famous discovery of my celebrated friend the Abbé Spallanzani.

The reproduction of the head of the snail does not observe so uniform a rule, as that of the head of those aquatic worms which I multiplied by sections 1741, and of which I published

[a] July 14.

lished an account a few years after [b]. The reproductions of the snail present a number of varieties, which it would be tedious to describe. Some instances Signor Spallanzani has given in his *Programma*, and after him I have mentioned them in *La Palingenesie*, part 9. I refer to those two works. Here I should confine myself to my own observations.

A profile of the anterior part of a snail, decapitated 8 May, and delineated 21 June, is represented a little magnified, Fig. 4. The two large horns, *g g*, begin to extend: the left is further advanced than the right, the origin of which is just visible. A brown and rather blackish line, *t*, proceeds from the large right horn; this is the optic nerve and its muscle, the various motions and structure of which, Swammerdam has exposed to our admiration. A degree of transparency is seen in the flesh; and as it encreases greatly in the snails that have fasted a month or two, the optic nerve and muscle are also much more evident. A white line, *l*, runs along the back: I am yet ignorant whether it is a vessel. The anterior part of the same snail, viewed from before, is represented from the life, Fig. 5. Only the higher extremity of the large horns is seen, and at the extremity there appears a minute black point: this is the eye of the snail, in which,

[b] Traité d'Insectologie, Paris 1745.

which, Swammerdam assures us, he has found the three humours of our eye, and two tunics, the uvea and arachnoid. Here the eye is already visible, although the horn only begins to grow. I have perceived it in horns that had made still less progress, as I shall immediately observe. The small horns do not yet appear. We know they have no eye at the extremity; neither are the new lips of the mouth, *b*, visible. This snail I shall design by the letter A.

The anterior part of another snail, drawn 23 June, is represented a little magnified, fig. 6. The reproduction is a degree advanced. One of the small horns, *p*, is very evident, and appears to be completely regenerated. The corresponding horn, has not begun to extend. Above the small horn, we perceive the origin of the large horns, *g g*, which has made but too little progress. Here is a striking example of the varieties to be seen in the regeneration of the head of the snail. One of the small horns has made the greatest advance, while the corresponding horn is not yet seen, and the large horns only begin to be observed. A profile of the same snail is seen, Fig. 7. The transparence allows the optic nerve, *t*, to be seen proceeding from the origin of one of the large horns: the eye of this horn is distinctly observed: the lips of the new mouth, *b*, are likewise visible. This snail I shall design by the

 letter

letter B. The anterior part, drawn 2 July, is represented a little magnified, Fig. 8. The plan is seen, Fig. 9. The mouth, *b*, cannot be mistaken. The large horns with their eyes, *g g*.

Another snail, which, upon 23 June, seemed to be completely repaired, shall be designed by C. The four horns were perfect, and had acquired the natural size of the horns of this species. The mouth appeared to be repaired: the opening complete, and the new lips very distinct, were of the figure and proportions they ought to be. In a word, this snail so much resembled other snails of the same species which had not been mutilated, that I could distinguish it only by the transparence and the diminution of size. It is represented, drawn from the life, Fig. 10. The section of the anterior part of the same snail is represented, Fig. 11. The new mouth and its lips are distinctly seen. Above it, at a little distance, there is seen, from the transparence of the flesh, an oblong spot, *t*: this is the teeth of the snail, which the lips can approach or recede from. These two figures were not drawn till towards the middle of Summer. After the 23 of June, I began to supply the snail with young vine and lettuce leaves; but it did not touch them. After traversing the leaves and the sides of the vessels for some time, it commonly fixed itself to the covering, and remained there

for

for whole weeks. Notwithſtanding it faſted more than two months during Summer, it always ſeemed to be in good health, and is ſtill well when I write this; 21 July.

I have ſaid, the eyes appear, although the large horns only begin to repair. This I ſaw in one of my ſnails decapitated 8 May, the head of which, ſeparated from the trunk, is repreſented, Fig. 1. The regeneration had made very little progreſs, 6 July; this day I cauſed it to be deſigned. Fig. 12, the plan of the anterior part. The origin of the large horns is ſeen; they do not yet begin to extend, and their place is indicated only by the eye, which is already perceptible; it appears like a black point, as ſmall as it is poſſible to make with the fineſt pen. The ſnail has been deſigned at the moment when extended to the utmoſt; and I have taken the ſame precaution with reſpect to all the ſnails deſigned. In that of which I now ſpeak, neither the ſmall horns nor the mouth yet appear.

When the ſnail retracts the large horns within, the black point or eye is eaſily perceived through the fleſh. Oftener than once I have diſcerned it with the naked eye, and even in thoſe ſnails whoſe reproduction is very little advanced.

I ſhould not neglect to obſerve, that, of the twelve ſnails decapitated 8 May, only one died.

died. All the rest seem to be well while I write this, 27 July; but the progress of reproduction is very various. In some, it seems to be but begun: in others, only the large horns are repaired, the origin of the small is not yet perceived, and the mouth is not well defined. The large horns of some of these are only half, or two thirds of a line long; while there are others, whose large horns are a line in length. Such are those of the snail, which I have hitherto designed by the letter A, the anterior part of which, as it appeared 21 June, is represented fig. 4.; and the anterior part of the same snail, as it appeared 26 July, is seen fig. 13. Something singular is presented by the large horns. They are thicker in proportion to their length, than those of unmutilated snails. At the extremity, we remark a kind of deformity, which seems produced by a certain plication of the flesh, giving the horns the appearance of being monstrous. The eye, however, is very distinct. The colour of the horns tends to violet, and is generally so in reproduced horns. It tends greatly to that of the nerve running through the flesh. The anterior part of another snail is seen, fig. 14. The mouth is not perceptible, but under the appearance of a little prominence, *b*. It seems awry.

I have in general, with Signor Spallanzani, remarked various conſpicuous irregularities in the reproduction of the double parts of the ſame ſnail. I ſee a large horn, of only half, or of two thirds the length of the correſponding horn: at other times, this one is ſcarcely viſible. I likewiſe ſee a ſmall horn completely regenerated, while its fellow is barely perceptible, or not at all. I ſee a mouth, one lip of which is only half reproduced, while the other ſeems completely repaired.

I confine myſelf to theſe few examples: they will ſuffice to give an idea of the varieties preſented by the regenerating head of the ſnail. It ſeems to me, that they infer the reproduction of one part to be independent of the reproduction of another; for, how can we reject this deduction, when we ſee one horn completely reproduced, while the reſt are inviſible, or only begin to grow? This fact will not fail to be conſidered as moſt important in the theory of thoſe admirable reproductions. But I will not touch upon it now, as I have endeavoured to gueſs at it in part 10. of the *Palingeneſie*.

I had, upon 12 May, decapitated thirty ſnails, of the ſame ſpecies, treating them preciſely in the ſame manner as the firſt: more than two thirds have periſhed. Thoſe ſtill alive, regenerate with various degrees of ſlow-

neſs,

neſs, preſenting the ſame, or varieties analogous to thoſe already deſcribed.

I ſhall obſerve upon this occaſion, that the months of May and June, and the beginning of July, have been very freſh and rainy. On ſome days of the firſt week of July, Reaumur's thermometer, at ſunriſe, ſtood at 4, 5, and 6° above freezing [a].

At preſent, I will not enlarge the diſcuſſion of my experiments upon the reproductions of ſnails farther: I intend to reſume the ſubject in another memoir. I think enough has been ſaid to prove, that nothing is more certain than this wonderful reproduction. I know not what to ſay of the fruitleſs attempts of ſome philoſophers, and particularly thoſe of Meſſ. Adanſon, Cotte, and Bomare. Perhaps they have too ſoon declared the ſtate of their experiments, or they have taken for an equivocal, what was a real reproduction; or perhaps they have thought to be dead thoſe ſnails ſtill alive. In this caſe much patience is requiſite, and, above all, to deſpair of nothing. I do not ſpeak of the diverſities which the difference of ſpecies might occaſion, in the conſequences of the experiments made by thoſe celebrated perſons; for I have reaſon to think, that among the great number of ſnails upon which they have operated, there were ſome of the ſame ſpecies as

[a] About 41, 43, and 46° of Fahrenheit's thermometer.

as mine. Neither do I ſpeak of the diverſities that might ariſe from the difference of climate; for the climate of Paris is very little different from ours. I therefore entreat thoſe able naturaliſts not to be diſcouraged, and to examine again a ſubject ſo pregnant with new truths; a ſubject which cannot be too deeply inveſtigated. They poſſeſs far more intelligence, talents and ability than are neceſſary to ſucceed in experiments of this nature; and I may predict the moſt complete ſucceſs, if they will not be diſcouraged, and if they will proceed in the manner I have done.

M. Adanſon wrote me concerning his own experiments, 30 July 1769.

"I begin to have a philoſophic doubt concerning the regeneration of the head, the "horns, and the jaws of ſnails. My experiments, diverſified to infinity for more than a "year upon between fourteen and fifteen hundred ſnails of different ſpecies, have convinced me that my doubt has foundation. I "have, as every one has had, reproductions, "even very ſudden ones, of horns, heads, lips, "and other parts, but thoſe were reproductions of parts which had not been completely cut away: for all the heads, (I ſay the "real heads), all the horns, all the jaws, and "the other parts that have been completely "cut away, and only a quarter of a line from

"the

" the origin, never have exhibited any kind of
" reproduction, far less a regeneration. Let us
" be strict, and seek for reality. All those
" who have mutilated snails, and first Signor
" Spallanzani, seem to me to have been de-
" ceived: they have thought they had decol-
" lated the head, when the cap only has been
" cut off: they have believed they have cut
" away, or eradicated, the horns, or the jaws,
" while the origin of them always remained;
" whence it is not wonderful there were re-
" productions. These, you will candidly ad-
" mit, are not reproductions, or rather rege-
" nerations, such as you, Mess. Trembley, and
" Reaumur, had seen in fresh-water worms,
" the polypus, and in the claws of crabs - - - - -
" How many well credited operations have
" deceived persons, less familiarised than we
" are, with similar operations, and the anato-
" my of shelled animals! They have imagined
" they had cut off, completely below the ori-
" gin, so many heads, horns, and mouths,
" which they have, in every journal and peri-
" odical paper, so liberally regenerated! I
" know well, how deficient we are in the
" greater part of nice experiments. Notwith-
" standing my great experience, I may almost
" presume to say dexterity, in the anatomy of
" the smallest animals, I always distrust my-
" self. For this reason, I have an hundred

" and

"and an hundred times repeated the same ex-
"periments, before hazarding the results to
"the public. I have laboured the first, or a-
"mong the first, to corroborate all the expe-
"riments of Signor Spallanzani, and to make
"additions to what might have escaped him.
"I have operated upon a greater number of
"animals, and diversified my experiments
"more than any other person, to judge by
"all that has been read before the academy
"or printed, and I am the only one who has
"read nothing upon the subject which I in-
"vestigate with the greatest assiduity - - - - - It
"is nearly the same with the reparation of the
"parts of newts, several species of frogs,
"toads, tadpoles, &c. I have seen sensible
"reproductions to the tails and feet partially
"cut away; but no regeneration, when those
"parts were cut off close at the origin. Consi-
"der well my expressions, *regeneration* and
"*origin*, upon which your principles rest so
"much, and there is no actual regeneration:
"and I hope you will do the justice to my
"doubts, as to acknowledge with me, that
"Signor Spallanzani and his followers have
"too far extended their expressions of rege-
"nerations, which were only reproductions of
"portions of parts" - - - - - -

To the numerous experiments and doubts of my celebrated correspondent, I shall oppose only

only the letter written to me by the Abbé Spallanzani, cited at the beginning of this treatise, wherein he details the precautions he used, to avoid error. I sent a copy of this letter to M. Adanson; but it did not produce upon his mind the effect I expected; and he still persisted in his doubts, when he wrote me, 20 July 1775—" The various parts cut or taken away, " not only from different species of snails, but " also from several other aquatic animals, as " frogs, toads, newts, have produced no or- " ganized reproduction to me, as the part cut " did according to Signor Spallanzani. I have " so much diversified the experiments which " my friend Mr Needham, and some other ob- " servers of this rank, have witnessed, that " we all esteem it certain, that whenever the " operation has been complete, the reproduc- " tion is but a stump, that is, a mass of flesh " unorganized, or differently organized: and, " Signor Spallanzani should know, that the " observations of our most celebrated anatom- " ists have proved, that the reproduction of " the tails of lizards, so common, although " externally well formed, present no regular " ossification as the rest, nor any vertebræ in " the interior." - - - -

M. Adanson is, as we see, one of those philosophers who start difficulties upon facts, and who wish themselves to see prodigies again

and

and again, before admitting them to be true. I cannot blame such a degree of reserve; but I confess, that in this case it appears to be extreme, especially after evidence so strictly demonstrative as the Abbé Spallanzani has given of his discovery. May I therefore hope, that the experiments I now publish, will triumph over the incredulity of our learned academician? Doubtless he will not suspect that I have only deprived the snails of the *cap*, to use his expression; for the head, so complete and so perfectly separated from the trunk, represented Fig. 1, will not permit the remainder of the least suspicion. I request M. Adanson to estimate all the details of my experiments, and to attend to the designs of my able artist, which represent so admirably well the regenerations which I have witnessed. I could with facility have extended them further; but I do not consider it as essential to the purpose I have in view. If it should be objected, that the snail, drawn from the life, Fig. 10. and 11, had not then touched the young vine and lettuce leaves with which it was supplied, I may answer, that it has given the most indisputable evidence of being provided with very good teeth: 27 July, it begun to eat the paper covering over the mouth of the vessel where it was confined, and voided some well formed excrements, the colour and consistence of

which,

which, exactly like paper, indicated that they were the remains of it.

M. Adanson also doubted the reproduction of the limbs of the newt, so perfectly ascertained by the numerous experiments of Signor Spallanzani; the principal results of which, he mentioned in his interesting *Programma*, published 1768. In his letter of 20 July 1775, above transcribed, M. Adanson says, "Whenever the operation upon the newt has been complete, there has only appeared the stump of a reproduction, that is, a mass of flesh unorganized, or differently organized:" and he cites the testimony of Mr Needham, and some other observers. But, what will M. Adanson himself say, when I inform him, that this pretended stump, or imagined lump of unorganized flesh, is the member itself, perfectly formed, concealed under those deceitful appearances, and which has been completely developed before my eyes, as I had formerly seen the developement of the heads and tails of those aquatic worms I multiplied by cutting in pieces? I have actually in my cabinet, newts completely repaired, of which I shall publish a history in a future memoir, accompanied by excellent drawings. Our celebrated academician has therefore been precipitate in his opinion, when he thought he only suspended it. He has decided, that the newt reproduces

duces but a stump, while this stump was the member itself, where there was nothing essential defective, and had only to acquire the size of that which it replaces.

M. Adanson was thus deceived concerning newts, as concerning snails; and the mistakes of such a naturalist, are a good lesson to those who have neither his knowledge nor his ability. I am persuaded that he will acknowledge his error; for I know him to be a sincere friend to truth, and I have not to fear that he will reproach me for having laid it open in this little treatise.

M. De Bomare, no less a friend to truth, and whose experiments had been as unsuccessful as those of M. Adanson, was, in consequence, equally incredulous. I had referred him to the same letter of the Italian observer, printed in the *Avant Coureur*, 30 October 1769; and he wrote to me 5 November 1775—" You " ask me, why I have not answered one of the " articles of a former letter, concerning the " reproduction of the head of the snail. I assure you, that all the experiments I have attempted upon this subject, seem adverse to " those of the Abbé Spallanzani. You will " see at the article Limaçon of my Dictionary, " edit. 1776, what I have said upon this subject, which I before mentioned in 1768." I shall transcribe from the *Dictionaire d'Histoire*

Naturelle, the passage to which I am referred by M. de Bomare.

"I acknowledge that, not being able to credit the reproduction; while at the Chateau "de Chantilly during Autumn 1768, I made "many experiments upon the subject, which I "have communicated to the public: the result "follows. Of fifty-two land snails, whose "heads I cut off, (all, whenever they felt the "cutting edge of the instrument, suddenly and "very powerfully contracted themselves: the "section finished, the part retiring precipitate-"ly into the shell, appeared wrinkled and "contracted, like the extremity of the rectum "of a hen), nine were in motion for twenty-"four hours, and only those where an imper-"fect cut was made upon the neck, between "the large horns and the organs of genera-"tion, the edge of the knife being so blunt, "that I had evidently seen all the horns retract "and retire into the interior of the animal. I "remarked, that in this way I had only cut "away the skin and the jaw of the snails, so "that in ten or twelve days they proceeded "from the shells, crawling about with mutilat-"ed horns. The snails from which I had cut "only the diagonal half of the head, displayed "only two horns; but those which I had sud-"denly decapitated entirely (which were by "far the most numerous) all died in a few

"days; excepting two which remained alive "five months fixed to a wall, and died in "Spring, without any indication of the repro- "duction of a head. I have taken other snails, "and made a longitudinal incision in the head, "between the four horns. Nature employed "more than a month in reuniting the two "parts, the animals appearing very languid. "I repeated those experiments 1769, and all "were unsuccessful. Many persons have writ- "ten to me from different countries, that "their attempts have been precisely similar to "mine."

It is singular, that I have succeeded with on- ly a dozen of snails, while M. de Bomare has failed with more than fifty, and M. Adan- son with more than fourteen hundred. But it is possible that those gentlemen were too anxi- ous to believe their experiments failed, or they did not pay sufficient attention to the progress of the regeneration, always more or less tardy, more or less disguised, and of consequence more or less difficult to recognise.

I have named another valuable naturalist, who has not been more fortunate than Mess. Adanson and De Bomare: I speak of F. Cotte, curate of Montmorency. He has narrated his unfruitful attempts in a letter to the Abbé Ro- zier, published in the *Journal de Physique*, May 1774. He denominates the reproductions of

the heads of ſnails, *imaginary*. He ſays, that from 1768 until 1774, he decapitated a great number of ſnails: that almoſt the whole died ſoon after the operation, which had been performed with a ſharp knife, not by drawing, but by a ſingle blow: and he concludes with three conſequences, which he aſſures us were attendant upon all his experiments and obſervations. 1. He has obſerved that ſnails have the property of contracting themſelves very ſuddenly, to protect the head from the inſtrument, in ſuch a manner as to eſcape with the loſs of only part of the horns, or, at moſt, the ſkin of the head. 2. When it happens that the head is actually cut off, it is not reproduced; at leaſt, he declares that he has never ſeen reproductions, not even of the parts of horns cut away. 3. Snails can live a very long time without eating, and without the head.

I know not whether Meſſ. Adanſon, de Bomare, and Cotte, have continued their experiments, or what has been the conſequence. But I can ſay, that I am not the only naturaliſt who has ſucceeded in corroborating the diſcovery of Signor Spallanzani: that has been already done by the celebrated Signora Baſſi of Bologna, by Meſſ. Lavoiſier and Schæffer. M. Senebier, paſtor and librarian of our Republic, who has given public teſtimony of his ſkill in philoſophy and natural hiſtory, has had the

the ſame ſucceſs with myſelf in experiments upon ſnails. I would here tranſcribe what he has written to me concerning them, did he not inform me that he has ſent his obſervations to the Abbé Rozier, to be publiſhed in his *Journal*.

Although the head of the ſnail is a very complicated little machine, as I have elſewhere ſhown, by the ſtructure [a]; it is indubitable, that the gelatinous quality of the fleſh greatly promotes its wonderful reproduction. I have enlarged upon this remark, when treating of the polypus. However, I do not mean that one ſhould infer that all gelatinous animals, and all animals in their firſt ſtate of jelly, may reproduce themſelves, or repair the loſs of their members, as the polypus and the ſnail. Experiment alone can diſcover the limits of this admirable property: and, what we have already learned concerning the extent of its dominion, ſhould excite naturaliſts to diverſify to the utmoſt their attempts upon a ſubject ſo fertile in wonders. I cannot too much exhort them to deſpair of nothing, not even in the moſt uncommon experiments.

[a] *Palingeneſie Philoſophique*, part. 9. New edit. of *La Contemplation de la Nature*, part. 3. chap. 21. note 4. 5.

MEMOIR II.

Having myſelf beheld ſome of the wonders which the regeneration of the head of the ſnail preſents, I wiſhed to know what M. Adanſon thought of the reſults of my experiments; and I requeſted him to read the preceding treatiſe which I had publiſhed in the *Journal de Phyſique*, September 1777. He did ſo, and wrote me 10 January 1778.

" I have read your excellent memoir upon
" the reproduction of the head of the ſnail,
" with all the attention it deſerves; and the
" peruſal only tends to confirm me more and
" more, of what a courſe of experiments, con-
" tinued nearly ten years, ſince 1768 until
" the preſent day, upon ſeveral thouſand
" ſnails, has informed me, namely, that the
" integral parts of the ſnail we ſpeak of, whe-
" ther the head entire, the eye, or the ocu-
" lated horn, the upper jaw radically extir-
" pated or cut below the origin, are not re-
" produced, either under the ſame form, or
" with the ſame organization as before. You
" have decollated two or three dozens of ſnails,
" 8 and 12 May 1777. I believe it. You
" cut

" cut them as I did myſelf, two lines below
" the origin; that is, towards the opening of
" the parts of generation. You have, as I
" have done, taken from each ſevered head,
" the upper jaw entire, and the oculated horn
" entire; and then, at the end of two or three
" months, in June and July, you have ſeen
" a third of the ſnails reproduce themſelves,
" even eleven of the twelve firſt decapitated,
" a complete head, with the eye-horns, the
" upper jaw formed like a horſe ſhoe, with
" the ſerrated teeth. You muſt permit me
" ſtill to retain my philoſophic doubt con-
" cerning the three laſt aſſertions, until you
" have repeated the following experiments up-
" on which it is founded; experiments which
" have ſerved to confirm the accuracy of my
" operations, and certain proofs that my ſnails
" were completely, and not apparently, decapi-
" tated. To have the ſame certainty, take
" the greateſt number of ſnails you pleaſe,
" (that is, hundreds, to provide againſt the
" great mortality that will follow), not of
" the ſmall ſpecies, called the Lacquey, which
" you have made uſe of, and which is moſt
" deceiving, from its great lubricity and agili-
" ty in evading the knife, an agility propor-
" tioned to its ſmallneſs, which muſt have de-
" ceived you; at leaſt, that happened to me
" in my firſt attempts, which obliged me to

 " relinquiſh

" relinquish it: take, I say, the large yellow-
" ish snail of the vine, named Pomatia, or ra-
" ther the brown garden snail, called by us
" the Gardener, which is almost as large, and
" the most common of all: after having kept
" the whole, immerged one or two days more
" or less in water, under a press, to diminish
" their vivacity and lubricity, tear away the
" upper jaw, which is formed like a horse
" shoe, and edged with five or six teeth, the
" lower palate, which is a membrane dentated
" like the tongue of a cat or a file, and era-
" dicate the two large oculated horns, using
" for these last little pincers, with thread or
" flax, to take away any edge, and preserve
" them from slipping, or indeed pressing the
" neck of the animal with two fingers, to era-
" dicate the jaws; avail yourself of this forced
" situation, to cut away, with a fine and sharp
" botanical scalpel, the two oculated horns,
" with the bulb below the eyes; or only one
" of the horns, to have an object for compari-
" son: cut the head entirely off others, ob-
" serving whether the severed heads have the
" jaws and eyes complete: preserve the heads,
" jaws, and eyes, to be certain that you have
" as many as snails operated upon. The snails,
" thus deprived of teeth, eyes, or heads, will
" for the most part live six months, even one
" or two years, without eating; they gradual-

" ly

" ly become emaciated unto perfect extinction:
" if, during this time, they recover new eyes,
" new jaws, a new head, which I have never
" had the good fortune to see in those identi-
" fied after the operation: if this experiment,
" made with all the precautions I have taken,
" and which I believe it necessary to use, suc-
" ceeds in your hands, and in those of the
" Abbé Spallanzani, I shall esteem it a fact,
" that the parts entirely separated are repro-
" duced in those animals. But take care,
" would you desire to make those contradictory
" experiments, without which you cannot be
" certain of the real reproduction of a jaw, an
" eye, or a head? I abridge the subject, because
" the consequences deducible are in my letter
" 30 July 1769, which I thank you for having
" reminded me of in your treatise - - - - -

" I pass to newts. I have not yet been able
" to procure the perusal of your memoir: but,
" excepting the tail, to judge by the observa-
" tions I have made upon that of lizards, it
" does not seem to be capable of reproducing
" osseous vertebræ; and although I have
" only had reproductions of stumps of the
" feet cut off several species of those animals
" and of frogs, because I have not been able
" to prosecute my experiments with so much
" conveniency, or so long, upon them as up-
" on snails, I very firmly believe the possibili-

" ty

"ty of the reproduction of the fingers and of "their bones, whenever the anterior or poste- "rior part of the arm is not cut off."

I want words to expreſs all the ſurpriſe excited in me, by this letter of my learned correſpondent; and I doubt not, that the reader will partake it along with me. What M. Adanſon deſires me to do, is preciſely that which would occaſion the failure of the experiment: for how could one tear out, or eradicate with pincers, the different parts of a ſnail, without cauſing the greateſt diſorder within? How, in this way, is it poſſible to ſucceed in eradicating all the parts? And, ſuppoſing it to be poſſible, would not we endanger the ſources of reparation? Is it not ſufficient, that I am certain, by the moſt attentive examination of the heads I have cut from my ſnails, that they contain all the parts that characteriſe a head, ſuch as the four horns, the mouth, the jaws, &c.? Was it neceſſary to cut out the large horns with a botanic ſcalpel, in order to be ſure that the ſnail would produce new ones? Was it not enough that I had ſeen, and many times ſeen again, the origin and progreſs of the new oculated horns, that I had ſeen the new eye and the optic nerve appear the firſt in this wonderful reproduction? It is improper here to encreaſe, as M. Adanſon has done, the agility with which the ſnail retracts its head the moment

ment it is touched by the inſtrument; for the agility is not to ſuch a degree as to prevent one, with a little addreſs, from performing complete decapitation. I can even ſay with truth, that I have very ſeldom failed to effect it, at leaſt when I have taken the precautions mentioned in my memoir.

M. Adanſon ſeems to reproach me with having employed ſnails of too ſmall a ſize: he ſays, the ſmall ſpecies I have uſed muſt have deceived me, from their great agility in eluding the edge of the knife. Neverthleleſs I can affirm, that I have operated upon the ſmall ſpecies with as much facility as upon the middle-ſized, and even upon the largeſt. But, we exaggerate the quickneſs of ſnails in ſaving their head from the inſtrument; for, an abſtinence ſeveral days, and the water in which they are immerſed, weaken them more or leſs, and to a certain degree diminiſh the celerity of their motions. Beſides, if I made uſe of ſmall ſnails, it was only becauſe I reaſonably preſumed, that the wonderful reproduction I wiſhed to behold would be performed more eaſily, or in ſhorter time, than in the largeſt ſnails. I have not neglected alſo to operate upon thoſe of the largeſt ſpecies, and I ſhall relate the effect.

To terminate the anſwer to the objections of our celebrated Pyrrhonian, I ſhall here ſubjoin

join an extract from a letter I wrote to him, 21 January 1778.

"If it was in my power, my dear and illuf-
"trious friend, I would make the experiments
"you defire upon fnails. But in truth, I do
"not think that any thing can be done more
"ftrictly demonftrative, than what has been fo
"well executed by my friend Signor Spallan-
"zani, and what he narrated at length in the
"letter of 11 September 1769, a copy of which
"I fent you, but upon the fubject you have
"never replied a fingle word.

"What is required? To afcertain whether
"the head is completely cut off, and whether
"the head reproduced has all the organs of the
"natural head. What muft be done, to af-
"afcertain thefe two facts? It is neceffary to
"diffect, with care, the head cut off; to exa-
"mine the interior with attention; and to be
"convinced that the head contains all the or-
"gans belonging to it: then, it is neceffary to
"diffect the reproduced head with the fame
"care; to be fure, by an exact examination of
"the interior, whether it actually contains all
"the parts belonging to the head of the fnail.
"This has been frequently done by Signor
"Spallanzani: and I now afk you, whether
"there is any foundation, in ftrict rhetoric,
"for doubting an experiment made with fimi-
"lar precautions? Yet you write me, 30 July
"1769,

"1769, that the Abbé Spallanzani muſt be "deceived; that he thought the whole head "was cut off, when the cap only was taken a-"way. You perſiſt in the ſame aſſertion, 20 "July 1775. Certainly you have not attend-"ed to the letter of the Reggian obſerver, to "which I referred you. Permit me to refer "you to it again.

"I have ſet apart all the ſevered heads: I "have obſerved the whole with attention: I "have ſeen the two large horns with their eyes, "the ſmall horns, the mouth, the lips, &c. I "have then ſeen new horns protruded; I have "ſeen the eyes of the horns, the optic nerve of "the eyes: I have ſeen a new mouth, new lips, "and new teeth appear in the ſnails, in the "ſame ſnails whoſe former heads I had ſet a-"part: I have ſeen ſnails that gnawed with "their new teeth the covering of a veſſel, and "I have ſeen them void excrements, in which "was found the matter they had conſumed. "What more would you deſire, my worthy "friend? And how can you write me, after ſo "many united proofs, that you ſtill retain your "philoſophic doubt? Can ſuch a doubt, ex-"tended ſo far, and challenging the moſt ac-"curate, reiterated, and demonſtrative experi-"ments, be termed *truly philoſophical?*

"Conſider, that the Abbé Spallanzani and "I, are not the only obſervers who have our-

"ſelves

"selves beheld the wonders seen in the repro-
"duction of the snail. The celebrated Signora
"Bassi, Mess. Lavoisier, Schæffer, Muller, &c.
"have seen and described them also. Are you
"willing to think that all those observers have
"been imposed upon, they who have given such
"ample evidence of their ability and accuracy?

"With respect to the reproductions of newts,
"you tell me, you firmly believe the possibility
"of the reproduction of the fingers and their
"bones, so long as the anterior or posterior
"part of the arm is not cut away. I am sorry
"that you should have written this, before per-
"using my memoir upon the *Reproductions of*
"*Newts.* There you would have seen, that I
"have cut away the fingers, the hands, the
"fore-arm, whole arms, feet, legs, and thighs,
"complete; and that all those members were
"perfectly reproduced by the animal. This
"would have induced you to bestow more con-
"fidence in the beautiful discoveries of Abbé
"Spallanzani upon snails and newts. My me-
"moir was printed in Rozier's *Journal*, for last
"November; and I wonder that you, who re-
"side in the same place where it is printed,
"have not procured it. The figures added to
"the memoir are very accurate, but the designs
"were superior to the engravings. It will
"present you with facts which I hope in a short
"time

" time you will not oppose. See, therefore, and " believe."

The letter written to me by Signor Spallanzani upon the mode of operating, and to which M. Adanson had not paid the attention it deserved, is so well adapted to convince naturalists of the reality of the ingenious discovery of which it treats, that I cannot dispense with transcribing it here, as the best refutation that may be opposed to the detractors of this discovery, and as a model of the method that one ought to pursue in researches of this nature.

" Modena, 11 September 1769.

" I thank you, Monsieur, for the intelli-" gence you have given me concerning snails, " considering the different results of natural-" ists, and particularly of French naturalists. I " am of opinion, that beside the little dexteri-" ty in the art of experiment, the diversity of " the species of the snails upon which they " have tried to repeat my experiments, has in " a considerable degree occasioned a differ-" ence in the result of their experiments. I " am certain, that all the snails of Modena " reproduce more or less; but I do not war-" rant the reproduction of foreign snails: some " among them perhaps do not possess this re-" source. Upon this point you will see great-" er detail in the preface I shall prefix to my

" Italian

"Italian translation of your *Contemplation de la* "*Nature*, which will appear this year. It is "very probable, that the snails which have ex-"ercised the industry of the learned anony-"mous Frenchman of whom you speak, are of "the number of those where the property of "reproduction does not reside in the highest "degree. I might say the same of the snails "upon which M. de Bomare and F. Cotte "have operated. But does it thence follow, "that I am deceived? To say I am, would be "a rash opinion, to call it no more. If any "one attempts to confute me, I will try to de-"fend myself; and my very circumstantial de-"tails, and those of my friends, will prove "that I am not deceived.

"You obligingly ask, whether the severed "head truly contained all the organs pertain-"ing to the head of a snail? To answer this "important question, I will mention what I "did in making the experiment. When I "saw that snails enjoyed the prerogative of "reproducing themselves, I begun to dissect "them, on purpose to obtain a perfect know-"ledge of the anatomy. I wished to make "myself master of all the organs of which "the head was composed. M. Lyonet's ana-"tomical work has always been my model; "and I was provided with all his apparatus. "I killed in water the snail I proposed to dis-

"sect;

" fect: it then proceeds from the shell, the " four horns are displayed, and it dies in this " position, which is the most favourable for " dissection. It is by this trivial expedient " that I have been able to convince myself, " that the head cut off truly contained all the " parts Swammerdam has described in his trea- " tise upon snails.

" It was only after having studied well the " structure of the head, and having ascertain- " ed the situation of each part, that I begun " to mutilate the animal; and I proceeded in " this manner:—Before cutting off the head " of the snail, I waited until it had come out " of the shell, and the horns were completely " displayed: then the operation succeeded " wonderfully well: and it often happens " that the severed head will keep the horns " nearly as much extended as before the ope- " ration, only they sink down and appear " feeble: the head itself contracts or concen- " tres very much. I have soaked it in water, " and in some hours it dilates, and becomes " soft; in which state it is easy for me to " anatomize it.

" I begun the dissection close to the cut; " and after dividing the integuments, I had the " pleasure to observe very distinctly the sepa- " ration or distribution of ten nerves, pro- " ceeding to the eyes and the other parts of

"the head: I obſerved as diſtinctly the diſ-"
"tribution of the nerves of the œſophagus,"
"and the muſcles ſerving for the different mo-"
"tions of the head. Sometimes, inſtead of"
"the nerves, I examined the whole or part"
"of the brain of the ſevered head.

"Continuing the diſſection, and extending it farther, it was eaſy for me to trace the different parts, even to the places where they were inſerted in the head. Without the leaſt difficulty I found the throat of the animal, its tongue, lips, mouth, teeth, the four horns, with their nerves, muſcles, and other appendages. If I choſe, I could retract the horns into the head: I had but to pull the ends of the divided muſcles.

"Theſe are, my dear correſpondent, the moſt remarkable parts that preſented themſelves to my view in the ſevered head. I ſay the moſt remarkable, for I diſcovered many other leſs important, of which I ſhall ſpeak in my large work.

"I now aſk you, If this aſſemblage of all the parts compoſing the ſevered head; if this aſſemblage, which I have ſeen and reviewed an hundred times, is ſimply the integument of the head, or part of this integument, as the French obſervers, whoſe opinions or experiments you have communicated to me, imagine: is it not the moſt ſa-

" tisfactory

" tisfactory evidence, that I have been under " no illusion, and that this severed head con- " tained complete all the organs which com- " pose it?

" I practised nearly the same method with " regard to the head reproduced. It would be " superfluous to give you a detail of the or- " gans composing it. I could only repeat, " word for word, what I have just said of those " we discover in the original head cut off. It " is true, that, oftener than once, this decol- " lation gives place to various monstruosities " of the parts reproduced: but this does not " affect the essence of my discovery.

" I have taken care to measure the severed " head, and to compare it with that repro- " duced. I have taken many other precau- " tions, which I now pass in silence, but shall " mention them at large in my work.

" I may flatter myself, that my treatise up- " on the reproductions of snails, will be so " rich in experiments, and that those experi- " ments will be so accurately described, and " so well detailed, as to convince the most ob- " stinate infidels."

The reader may now judge, if I had any foundation for reproaching the excess of M. Adanson's pyrrhonism concerning the discovery upon snails. It is in truth most singular, that he persists in his doubts, after the perusal of a letter

letter so strictly demonstrative as that I have just transcribed. How many physical facts are admitted by philosophers, and by M. Adanson himself, which are not better attested than that of which we speak! Shall I say more? M. Adanson still retained his doubts, 9 October 1779, as I learned from himself, during a visit he then paid me, after a journey made for the recovery of his health. At that time, I had not snails in complete reproduction; but I had the satisfaction of convincing him, by the testimony of his own eyes, of the reality of the wonders presented by the reproduction of the members of the water newt. I shewed him newts in various stages of reproduction. I shewed him arms, hands, thighs, legs, feet, perfectly well formed. He yielded to so many accumulated proofs, and was convinced that what he had erroneously supposed simple stumps, were in fact real members, which would be completely regenerated.

I return at present to the experiments I have made upon the reproductions of the head of snails. Those whose progress I mentioned in the first part of my treatise, died before finishing the reparation of the head. They became much emaciated, and assumed a transparence that is unnatural to snails in the country. One, whose anterior part is represented fig. 12, reproduced only one of the large horns, about a

line

line in length, but much thicker than the large horns are when they begin to extend. This ſingular horn, which ſeemed to be formed like a ſort of ſpindle, had two eyes, *y y*, very diſtinct, and each eye had its optic nerve. Fig. 15. repreſents the whole magnified, where, ſlightly ſhaded, the part reproduced is indicated, which is always of a clearer colour than the original fleſh. The horn being cloſely examined, it was eaſy to diſcover that it was formed by the union of two horns, which were ingrafted in each other, as in a manner by uniting. There was no trace of a mouth, nor of the ſmall horns, perceptible in this ſnail: how then can a complete decapitation be doubted?

I reſumed thoſe experiments upon different ſpecies of ſnails, Spring 1778. Their various reproductions preſented varieties ſimilar or analogous to thoſe I had obſerved in the ſnails decapitated the preceding year. There was among thoſe varieties, one like that of which I have juſt ſpoken. Two eyes at the extremity of the left horn, were very diſtinctly ſeen in a ſnail that had begun to reproduce the mouth and the two large horns, fig. 16. The anterior part of the ſnail is here repreſented larger than life.

I continued to attend my ſnails during the whole courſe of the year 1778. The progreſs

of

of regeneration was as usual very unequal, and none repaired the whole head.

I decapitated twenty-four snails of the same species 26 May 1780, confining them in vessels after the operation. Several reached the summit of the vessel, and attached their shells to the sides, or to the paper covering over the mouth. The greater part shut up the shell with a very thin operculum, at different degrees of depth within.

Having immerged some of the decapitated snails in water, 9 September 1780, that I might judge of the state of the reproduction, there were two among them that presented a remarkable monstruosity. In one, I observed only one large horn, very like that of fig. 15, and evidently appeared to be formed as if by the union of two horns. At the extremity, were two small shining black eyes, each provided with an optic nerve perfectly visible through the transparent flesh. This monstrous horn, which was about a line long, appeared thicker in proportion than that of fig. 15. It greatly resembled a spindle, being cut even at the extremity, and being all nearly of the same thickness. But it was different from the other, by a more important distinction. On the left side was seen, a little under the eye, a very minute tubercle, which seemed to be a second horn growing out of the large horn. In this

snail,

ſnail, I ſought in vain for the parts conſtituting the mouth. I could diſcover no trace of it: nor was there the leaſt veſtige of the two ſmall horns.

The ſecond ſnail preſented a monſtruoſity of another kind; only one large horn had protruded. At the extremity I thought I could perceive three black eyes, but ſo cloſe to each other, that they ſeemed confounded together. Fig. 17. 18. the figures are magnified. Upon the ſuperior part of the horn were ſeen, very diſtinctly, three parallel optic nerves, only one of which proceeded to the three minute eyes. Under this horn, and at a little diſtance from the origin, a very ſmall one is diſcovered, which ſeems only beginning to expand. In this ſnail, as in the former, no mark of the parts forming the mouth were viſible.

I obſerved again, both with the naked eye and a magnifier, the two ſnails, 25 October. They had made a very ſenſible progreſs. Upon this day, only eight of my ſnails remained alive: all, of that ſpecies whoſe ſhell is yellow or yellowiſh. The other ſixteen had periſhed; ſome ſooner, ſome later.

I immerſed in water the ſix ſnails whoſe reproduction I had not yet examined. The greater part had made but little progreſs, and ſhewed only the origin of a ſingle horn. One alone had protruded two eye-horns, at leaſt a line

line and a third in length; the optic nerves so large or so visible, that they seemed to darken the greatest part of the horns; but the small horns, the lips, or the other parts of the mouth, did not yet appear. This snail persisted in remaining in its shell, although immerged in water more than two hours. Suspecting it to be dead, I had taken it out, and, only after the lapse of several hours, it agreeably surprized me, by proceeding from its shell of its own accord, and displaying to my eyes its new productions.

In another Memoir, I will give the history of my eight snails.

After making experiments upon snails of the smallest species, it was proper to make them also upon those of the largest species. This I begun to put in execution 24 May 1780, upon twelve of the largest snails of our country. We may judge of their size, compared with that of the snails which have been the subject of this and the preceding Memoir, when I say the diameter of the opening of the shell of those was at least nineteen lines, and that of the shell of these but four or five.

In some weeks, the half of my snails died, and exhaled an excessive fœtid odour. Upon 13 August, I immerged the six surviving snails. They proceeded from the shells, and I saw that the large wound was perfectly cicatrized, but could perceive no indication of reproduction.

Towards

Towards the middle of October, other two snails died. Upon the 18, I immersed the four remaining snails in water, where they were more than three hours without appearing. I scratched with my nail the last volute of the spiral of the shell in vain: all my attempts were fruitless, and the snails obstinately concealed the anterior part: they were then taken from the water, and shut up in their vessel. I will ingenuously confess, that I had little hope that the snails had made new productions. What then was my surprise, when they next morning proceeded from their shells of their own accord, and exhibited unequivocal evidence of reproduction, and even of reproduction pretty far advanced! One, which begun to repair the head, had two large horns about a line in length, fig. 19. The left horn, *c*, was thicker than the other, had two distinct and black eyes at the extremity. The right horn, on the contrary, appeared to have none. I could as yet discover no indication of the regeneration of the small horns, and of the mouth.

Another snail, fig. 20, had likewise begun to reproduce two large horns, nearly of the same size as those of the preceding: but the right horn appeared monstrous; it seemed to terminate by three small soft points, which are drawn from the life.

The reproduction of the head of a third snail was announced by four or five very short protuberances, where the large left horn only could be distinguished, presenting three black points, or three very minute eyes. The whole is magnified, fig. 21. The three eyes, *a*, *b*, *c*: two, instead of being situated at the extremity of the horn, are situated upon the side: the eye, *a*, is more apparent than the eye, *b*; the third, *c*, is a little below the other two. It was impossible for me to see the optic nerves through the flesh: the reproduction had still made but little progress. Under the horn are seen two protuberances, *p*, the nature of which cannot yet be known.

I shall continue my observations upon those large snails, and give the sequel in another Memoir.

FINIS.

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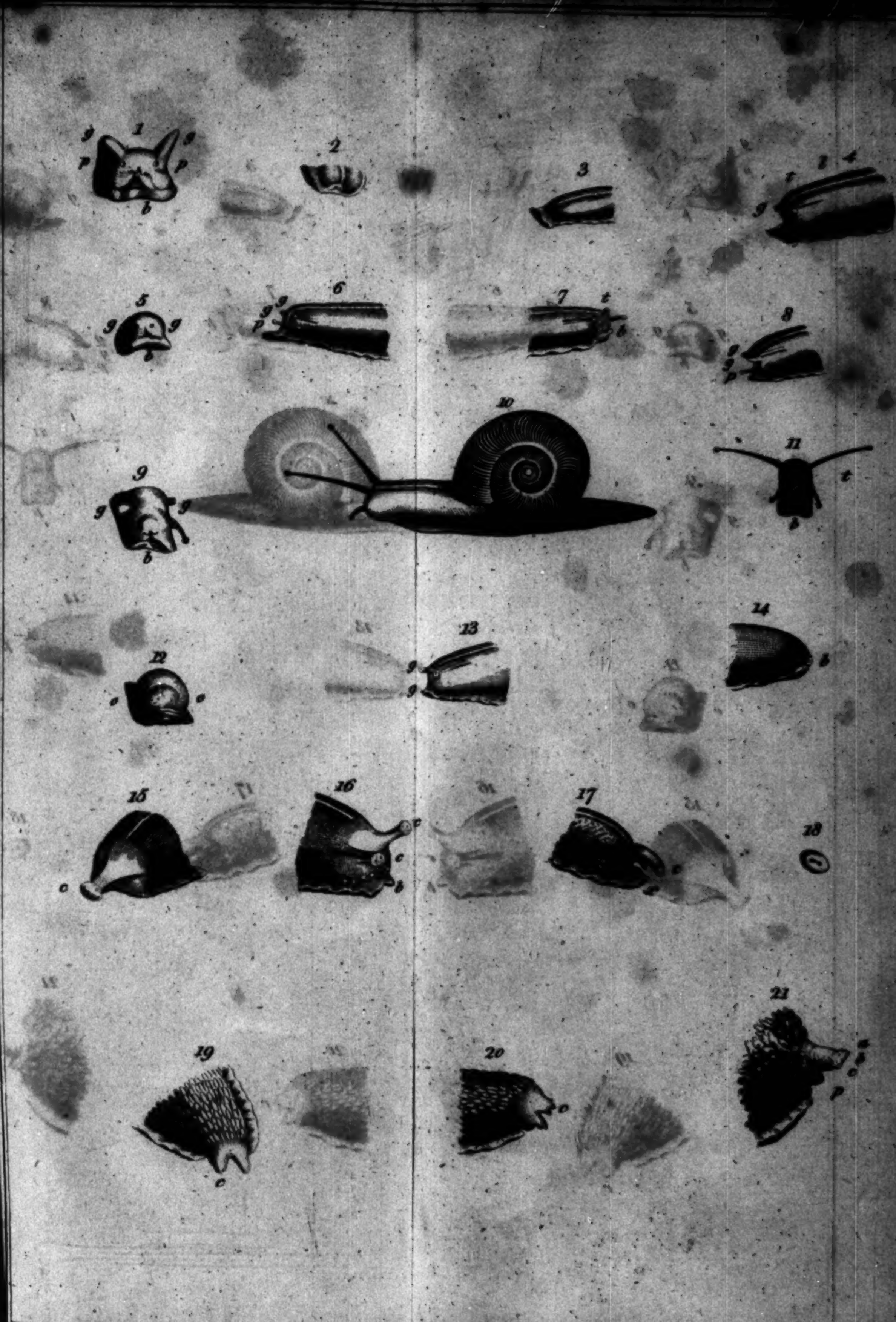
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